FARADAY'S DIARY

VOL. II

THE ROYAL INSTITUTION OF GREAT BRITAIN

From a water colour by T. Hosmer Shepherd, painted about 1840

FARADAY'S DIARY

Being the Various Philosophical Notes
of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820–1862
and bequeathed by him to the
ROYAL INSTITUTION OF GREAT BRITAIN
Now, by order of the Managers,
printed and published for the first time,
under the editorial supervision of
THOMAS MARTIN, M.Sc.

with a Foreword by
SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. II

AUG. 25, 1832–FEB. 29, 1836

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PREFACE TO VOLUME II

THE part of the Diary printed in this volume covers the period August 25, 1832, to February 29, 1836, and is contained in Folio Vol. II and part of Folio Vol. III of the manuscript. There is no break at the end of Folio Vol. II, which finishes, indeed, in the middle of a sentence, during the course of a description of electrostatic experiments on January 15, 1836. The entry for the day is concluded in the first few pages of Folio Vol. III.

The text has presented no new problems. Only a few isolated words have been found illegible, and punctuation, abbreviations and other peculiarities have been treated on the same general lines as in the first volume. Most of the illustrations have been reduced in size, generally to three-quarters of their original dimensions, and placed in the margin as far as possible, against the paragraphs to which they relate. Those which it has been necessary to place at the foot of the page are referred to by signs at the beginnings of the paragraphs, except in a few cases, where the context requires the reference sign at a particular point in the text.

An abbreviation which makes its appearance for the first time in this volume is "sulc." for sulphuric. In the manuscript there is a tendency, about half-way through, to drop the colons from "c: i:", for cubic inches, and to use full points instead, but in print the colons have been retained throughout. "Anion" and "cation" are sometimes spelt with the "i" modified, e.g. "anïon" and "catïon" at par. 2765.

T. M.

ROYAL INSTITUTION
September 1932

CONTENTS

In this volume the unbroken series of paragraph numbers begins which runs nearly to the end of the Diary. In the Contents the paragraph numbers (in black type) are given as well as the dates.

Cross references to published papers are included as before. The part of the manuscript here printed contains the notes and records of experiments drawn upon by Faraday in writing Series III to X and parts of Series XI and XII of the electrical researches, as well as certain other shorter papers. All of these will be found in Vols. I and II of the collected *Experimental Researches in Electricity*, to which the page references have been given.

In some cases it has been difficult to decide in which of the published papers a particular entry in the Diary has been used, or if it has been used at all. No cross references have been given in cases where the connection is indefinite or doubtful. As in Vol. I, there is a considerable number of entries of which no published result can be traced.

No exact correspondence between the passages in the Diary and in the printed works can be expected. The cross references should be used as a general guide to the papers in which may be found, more fully developed, ideas of which the Diary contains the first suggestions and the day-to-day record of the relevant experiments.

In the first reference to each of the Series Faraday's own sub-headings have been given in full, since it may be of assistance to the reader to have included in the Contents these descriptions, in his own phraseology, of the succeeding aspects of his work.

FOLIO VOLUME II OF MANUSCRIPT

1835

FOLIO VOLUME III OF MANUSCRIPT

PLATES

FOLIO VOLUME II
OF MANUSCRIPT

AUG. 25, 1832.

1. Experimented with large E. Machine for deflection of galvanometer by its current. Plate of Machine feet in diameter.
2. Have arranged a good discharging communication with pipes, earth, etc. etc., so that when it is connected with delicate Electrometer and the latter with the machine, the most powerful working of the latter will not affect the Electrometer (Singer's Gold leaf).
3. Connected one end of Galvanometer wire with this communication and the other end with a wire and fine point. Charged large battery of 15 Jars and brought point suddenly and slowly up to it so as to cause continuous discharge—but could not affect the Galvanometer.
4. Occasionally needle moved a little but found that motions were always due to electricity excited upon the glass of the Electrometer jar. When machine is worked, a point in the room at considerable distance will cause motion of needle by rendering the glass electric. Or if wire of galvanometer is not perfectly connected with discharging medium, same effect. I think also that silk about it may occasionally become electric.
5. It is necessary to avoid passing a shock through galvanometer for otherwise the magnetism of the needles is disturbed or altered and the instrument deranged.
6. Colladon on deviation of Mag. needle by common Electricity of Machine or storms. Ann. de Chimie, 1826, xxxiii, p. 62.

AUG. 27TH.

7. Have coated galvanometer jar with tin foil inside and outside to a certain height, connected these with the discharging train (2) and then hung a strip of metal leaf within the jar by a wire through a cork, the wire being also connected with the discharging train. Now connected a point (needle) with the large machine by a wire —use an insulating handle—and brought the point towards the jar, the machine being at the same time at work. So long as the point was about the coated part of the jar there was no attraction or motion of the metal leaf within, except when very low, and that

was by a part of the wind issuing from point being introduced beneath the edge and acting as wind only. But when point near upper uncoated part of jar and the leaf raised, then strong attraction of it from electricity communicated to glass.

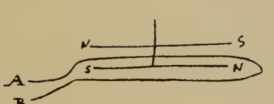
8. Made a frame work of wire and soldered needles on to it; this rested on uncoated part, the points projecting outwards. When machine point brought to within a foot of these, there was *no effect* on the leaf within; if brought quite within and between points of frame, then the glass could be charged a little and indicating leaf within moved. But so strong a case could never happen in expts.

9. Hence have now a galvanometer which will not be affected by common electrical attraction and repulsion.

10. When galvanometer coil is connected with the discharging system, know that it will not shew any common attraction and repulsion by electricity of machine (2).

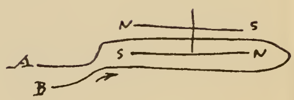
11. Now exptd. by passing electricity of battery through water. A thick thread about four feet long was thoroughly wetted and then one end attached to the A end of the galvanometer, containing 36 feet of wire, and the other end to a discharger. The end B of galvanometer wire was connected with the discharging system (2). The battery of 15 Jars was then connected, its outside with the discharging train, which is the same thing as being connected with the end B of the galvanometer, and its inside with the Electrical machine. A Henly's Electrometer was put upon the general conductor and the machine worked and the battery charged until the electrometer stood at about 40° or 35° of inclination with the stem. Then the discharger was brought in contact with the conductor and so the discharge made through water in the thread and through the galvanometer in the direction from A to B.

12. On these occasions the needle was deflected, and by continuing the machine at work and discharging the charge in the battery each time the needle in swinging returned in the direction of the first impulse, the deflection was soon raised to 40° or more.



13. This effect was repeated again and again successfully and the deflection of the needle was always in the same direction. The upper needle of the galvanometer was the strongest and the arrangement as in the figure, N and S meaning north and South end when in natural position. On making contact, i.e. sending current

from A to B, the upper S end went towards the east. This is precisely the kind of deflection that a voltaic current would cause.
14. Then the battery was separated from the machine and the effect of the machine alone on the needle was observed, i.e. the machine was worked and the discharger held against it whilst the needle swung in one direction; then the discharger was removed as the needle swung back and the machine meantime connected with the discharging train and also stopped, and then as needle swung in first direction the machine was reworked and the electricity from it sent through the wet thread, galvanometer coil, etc. as before. This was repeated several times and the deflection occurred and was increased each time until it was 30° or 40° or more.
15. The current on these occasions merely passed through the water and galvanometer coil away to the earth and not back to machine.
16. The direction of the deflection was the same as before and as every thing had remained in the same position gave consistent result. Hence mere machine current can thus deflect the needle.
17. Then changed the wires from the machine and the discharging train the one for the other at the galvanometer, so that the current now went from B to A.



18. On using the machine current only as it passed from B to A, it deflected the needle, causing the upper S end to pass now to the west instead of the east, as ought to be the case if true electro-magnetic effect were the cause.
19. Retaining the communications as last described, the battery was now put in use as before, so that its discharge also went from B to A, and the deflection occurred powerfully but contrary to what happened at first with the battery, but consistent with the changed state of the communications. Upper S end of needle went westward.
20. Now used a thick wet string instead of the thread and about the same length; effect was equally good and about the same.
21. Shortened the string, but with equal charge of the battery; the deflection still about the same in extent.
22. Used four short thicknesses of string instead of one long one; effect not apparently different.
23. When a thread is used the battery charge sinks gradually,

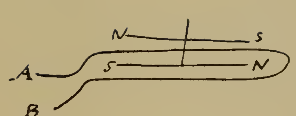
requiring perhaps 2 or 3 seconds to go down. When a short thick string is used it passed at once as a spark. But the effect on the needle is apparently the same. When it passes thus as a spark, wet string being in the circuit, the sound is very little and the spark had more of the appearance of flame than the ordinary spark, as if it lasted longer and was redder. It reminded me of the appearance of the discharge sometimes of a voltaic trough from points of charcoal.

24. On using an ordinary sized wire as connector between the galvanometer and the conductor of the machine and (throwing the battery out of gear) sending the current from the machine through it as had been done before through the water, I could [detect] no distinct effects upon the needle, but I must repeat the effect.

25. I rather doubt Colladon's actions of points, for I do not see why, if the galvanometer is sensible enough to shew effects with water, it should not shew effect if any of points, and yet I could not get such effects; but I must repeat the experiments.

26. I tried to make a discharger of fine needle points that could discharge the battery without a spark and yet suddenly, but I could not. A single point, unless approached very slowly and carefully, could not without a sharp spark. Nor could four points attached to wires about 3 inches apart but connected together at ends do it. A point requires much time to discharge from large surface and easily takes a spark from low intensity of battery charge.

AUG. 30TH.



27. Repeated the water expt. (18) with machine current only (i.e. not using the battery) and sending the current through the galvanometer from A to B. Effects were as before, and as current passed, the upper S end went eastward as on former occasions.

28. Now removed the string and used wire throughout, i.e. from discharger through the galvanometer away to the discharging train. Worked the machine and made and broke contact with the prime conductor alternately every 18 watch beats (that being the time for each swing of the galvanometer needle to the right or to the left). In this way accumulated effects and soon obtained deflection

of needles. Hence water is not necessary for wire alone does it. As current passed from A to B, the upper S end of needle went eastward.

29. Then repeated the water experiment, reversing the direction of the current, and now the upper S end went westward instead of eastward.

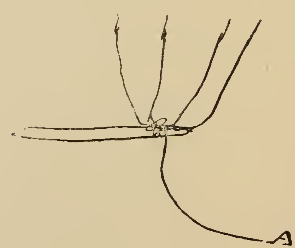
30. Then repeated Battery experiment (11, 19, etc.) and obtained an excellent swing of the needle with one discharge. The direction was the same as before, i.e. current from A to B and S end of upper needle towards the east.

31. The appearance of the spark between the prime conductor and metal discharger very remarkable when the charge is thus passed through water. The spark yellowish, slow, flamy, about $\frac{3}{4}$ of inch long, with little or no noise and in appearance, etc. very like a voltaic spark.

32. Then put a piece of box wood charcoal on to the prime conductor, and another piece on to the discharger, so as to pass the discharge between them and yet also through the water of the wet string included in the circuit. When thus retarded, the discharge of the battery gave a quiet spark at the charcoal, but it was very luminous and bright upon both surfaces of the charcoal, much resembling there the brightness of the voltaic discharge upon similar surfaces. In the rest of the course of the spark, i.e. the intervening distance, it was not so bright.

33. When the discharge not retarded by water was taken upon the charcoal surfaces, the spark was of the ordinary kind but very bright upon both surfaces of charcoal (resembling in that respect the voltaic effect), and the noise was loud and sharp and ringing.

34. When water formed part of the circuit (8 inches of wet string) then the shock of the battery charged by eight turns of the machine was such as I could easily support, caring indeed nothing about it. But when no water was there, I could not well bear four or five turns, the sensation then being general through the arms and chest and very much indeed greater than the former.



35. Now tried if currents drawn by points would affect the galvanometer. The end B was therefore connected with the discharging system and the end A by a long wire with a kind of fork consisting of four prongs of copper wire each terminated by a

very fine needle; the needles were about 2 or 3 inches from each other in a line. A stick of wax was used as an insulating handle but I do not believe it was necessary.

36. The machine was worked and these points held near it for 18 watch beats (28), then removed and the machine stopped and uninsulated for the same time; then the action of the machine and points renewed, and so on 5 or 6 times. The needle soon vibrated sensibly, and as the current thus gathered by points went from A to B the upper S end went as before eastward.

37. Then put a jar with a brass summit on to the air pump, and having exhausted it until the discharge of electricity through it was ready and in purple streams, sent that stream through the galvanometer. The needle was deflected just as well as in former cases.

38. Hence the current, whether transmitted through water or wire or exhausted air or collected by points, is still able to deflect the needle; the only requisite point being apparently to allow time for its action. But imperfect conductors, as water, brine, acids, etc. etc., are very excellent for their power of converting the charge of a battery into a feeble spark or rather a continuous current.

AUG. 31.

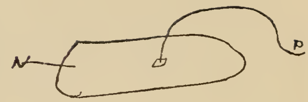
39. Experimented on decomposing power of common electrical current. Repeated Wollaston's fourth experiment (Phil. Trans., 1801, p. 429). The silver wire was about 1/100 of inch thick, coated and cut as there described and the ends put into a drop of solution of sulphate of copper about 1/10 of an inch apart. The current from the machine was not transmitted in sparks but by a piece of wet thread about 8 inches long, the rest of the circuit to the discharging train being metal with the exception of the drop of sulphate of copper.

40. After 200 turns of the machine the two wire surfaces in the drop were examined; that connected with the prime or positive conductor was discoloured and blackened, but it did not appear cupreous or burnish red; it was due to acid action on the silver. The other or negative surface had copper precipitated upon it, very evident both before and after burnishing.

41. Repeated the experiment very successfully.

42. The decomposing action is however very minute—very little copper is precipitated and no sensible trace of silver appeared in the solution of copper.

43. On using platina spatula as negative surface, a wire point as the positive, and putting between them a little bit of paper moistened with sulphate of copper, I obtained no sensible action after 400 turns of the machine.



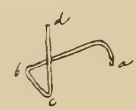
44*. The discharging medium is not so good but that when its end is brought within ½ an inch of prime conductor and sparks then taken by means of it, other sparks may not be drawn by the hand or a leyden jar from *a* or *a* at the moment the principle spark passes. It will be better to connect it still further, as with street gas arrangements or supply water pipes.

45. It would seem that water may entirely insulate a very weak electricity (De la Rive) or it may allow it to pass without causing or suffering any chem. action (common Electricity, etc.) or it may rise higher and then decompose (both common and Voltaic Electricity); and what is true of water is most likely true of all other moist or bad conductors if the electricity be proportionately diminished, and may be even true of metals themselves.

SEPTR. 1.

46. Experimented on chemical action of common electricity in another way and obtained beautiful results.

47†. Bent two small platina wires twice at right angles nearly, thus, so that the part *cd* would be nearly upright whilst the whole stood upon the three bearing places *a*, *b*, *c*. Put a glass valve or plate on a table, placed a piece of tin foil on that and then let the end of a wire connected with the electrical machine by 8 inches of wet string rest on the tin foil. Then one of the bent platina wires was so placed that *b* and *c* rested on the other end of the same tin foil and *a* on a second glass plate. The second platina wire was placed with its end *a* near *a* of the former and the rest in connection by tin foil or otherwise with the discharging train. Thus the connection could be arranged or broken at pleasure, being discontinued only between the two points *a*, *a* when the experiments were in progress. In this way



* [44]

† [47]

surfaces of contact as minute as possible could be obtained at pleasure, as will soon be seen.

48. A mixture of half pure Mur. acid and half water was rendered deep blue by sulphate of indigo and then a moist strip or place made with it on the larger glass plate and the two ends *a*, *a* arranged so as to be about ½ an inch apart and connected together by it. Immediately on turning the machine a decolouring effect was seen at the end *a* connected with the machine, i.e. the positive side, and after 20 or 30 turns the bleached portion of solution was so large and powerful that when the wires were removed and the whole of the solution stirred together the chlorine evolved by the machine was enough to bleach the whole.

49. Then connected those two ends *a*, *a* by a little solution of Hydriodate of potash mixed with a little starch. In a few turns of the machine the blue tint indicating iodine was evident at the positive pole. In these experiments with fluids on glass it is useful to put a piece of white paper under the glass and also to raise the latter in the air to get rid of shadows.

50. Then put a little bit of filtering paper moistened with the mixed hydriodate of potash and starch under the ends *a*, *a*, and put them about ⅛ of inch apart; the decomposing action was sensible when only half a turn of the machine was taken.

51. The paper very valuable; it makes contact by very minute surface, keeps the decomposed matter at the spot, and by its whiteness well shews the effect of change of colour.

52. The paper moistened with sol. hydriodate of potash alone is a very excellent test. The brown spot produced becomes blue by being touched with starch.

53. A piece of litmus paper was moistened with brine and submitted to the points *a*, *a*; the positive *a* soon made a red spot: 20 or 30 turns of the machine was enough. But this test is not so good as the hydriodate of potassa.

54. On moistening litmus paper with dilute Mur. acid and putting it under points *a*, *a* the positive one quickly bleached it.

55. Now moistened a piece of turmeric paper in sulphate of soda and placed it under the points *a*, *a*. In two or three turns of the machine a red spot appeared under the negative *a* and in 20 or 30 turns it was very good effect. On reversing the paper and

putting the spot under the positive *a* it disappeared, whilst another was formed under the negative *a*. This a very good test, but not equal to the Hydriodate of Potassa.

56. Now combined the litmus and turmeric papers, folding one piece over another, moistening both with sul. soda and so placing them on the glass that the litmus should be under the positive *a* and the turmeric under the negative *a* point. A few turns of the machine produced both effects, i.e. acid and alkali at the positive and negative poles.

57. All these experiments of to-day were made using the wet string in the circuit. But now repeated them without the string, i.e. wire reached from the prime conducter to the end of the platina wire so that the metal was continuous from the positive end *a* to the machine. The effects were quite as good as before.

58. Repeated the exp. (48) in form but put solution of sulphate of copper on glass between the ends *a*, *a*. 20 or 30 turns sufficed to precipitate the copper on the negative end *a* so perfectly as to make it look like copper.

59. On repeating this experiment but instead of the negative end *a* using the curved surface of the wire bent round into a loop, so as in fact to have a larger surface of contact, 100 turns of the machine did not cause any sensible precipitation of copper. This shews the value of concentrating the effect by diminishing the surface, but whether the effect was insensible because the new surface was so large as to dilute it or whether it was so large as to prevent the current ever rising so high in intensity as to decompose, I am uncertain of. I think the latter possible (see 45), the former most likely.

60. The decomposition of sul. soda and evolution of alkali and acid at the poles on litmus and turmeric paper occurred equally well, I think, whether the electricity was taken from the machine through wet string or directly by the wire in contact or by a series of sparks, provided that in the latter case the sparks did not strike across from positive *a* to negative *a*, but were conducted by the solution between. Hence I do not think that sparks are required to produce decompositions which currents cannot.

61. When the electricity struck in a visible spark from *a* to *a* across the moistened litmus and turmeric paper, the course of

the spark left a red mark over the litmus, but none over the turmeric. This red mark I believe to be due to nitric acid formed by the detonation from the nitrogen and oxygen of the air. Must try this. If the spark passes only part of the way across like a brush, still as far as it passes it reddens litmus. The effect must not be confused with the decomposing power of the current.

62. Have increased discharging system by taking in the water pipe and consequently all the water mains, etc. etc. Still the effect before described (44) takes place.

63*. Experimented with Wollaston's wires in globe; surfaces small and good. Water only in globe. Using sparks at machine, i.e. taking sparks about $\frac{1}{4}$ of inch long from machine by wire, then gas at both water surfaces. But this gas was evolved by an explosion in the water, for the spark was visible and the water was dashed about, as was very evident by the bubbles formed being knocked up, down and sideways at the moment of explosion and rising upwards by their levity afterwards.

64. When instead of transmitting the current in sparks it was transmitted as a constant stream, then the gas evolved at the water surfaces diminished very much, although the quantity of electricity from the machine was the same in the same time.

65. When the current was transmitted with as little interruption as possible, then merely a very fine current rose from the negative surface and a bubble or two now and then from the positive. But I think all the gas or nearly all the gas thus produced was still the result of little explosions, and not of true chemical decomposition by different attractions of two electrified surfaces. I do not think these decompositions are to be compared to those of the litmus and turmeric on the paper.

66. When instead of connecting the discharging train with its appropriated guarded platina wire it was dipped into the water to some extent and then the electricity passed in sparks, then gas rose as before from the remaining positive water surface, but none rose from the surface of the negative immersed wire, although the action was continued long enough to have covered it with fine film of bubbles if it had been necessary that the development on each pole should be proportionate. Hence in fact the general effect of evolving gas in such cases is due to the passage of the

* [63]

electricity at the spot and does not require an equal action elsewhere. It is not a case of usual electro chemical attraction.

67. On filling the globe with strong solution of Sul. Soda, then in passing the electricity either as stream or as sparks there was no gas evolved at either water surface, unless indeed the sparks were *very powerful*, and then the gas or air evolved was hardly sensible. In this expt. the concussion at the contact of the solution and wire was hardly sensible, so good is the conducting power of the former, and to this cause and perhaps also to the absence of air from the solution is due the absence of currents of gas or air in this form of the experiment.

68. Shall try water well boiled and purged of air.

SEPTR. 3RD.

69. I find that paper moistened with solution of hydriodate of potassa is affected by both points *a*, *a* if the electricity pass by sparks, however minute; for iodine is then set free at both wires. Hence care required in its use for electro chemical decomposition that sparks or even strong brushes do not pass.

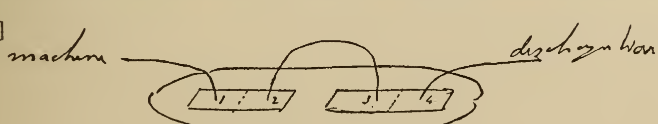
70. Sparks from either point *a*, *a* on to moist litmus paper will also redden it after some time. It is necessary in obtaining this effect readily that the litmus paper be not very alkaline.

71. This not the case with moist turmeric paper; it shews the effects of the poles fairly.

72. Connected the piece of litmus paper on which the positive *a* rested with the turmeric paper on which the negative *a* rested (   ) by a piece of wet string above four feet long. On working the machine the right decompositions took place at both poles, as readily I think as if they had been close together.

73*. Then varied the expt. by interposing a piece of platina wire (short) between the ends *a*, *a*, so as to give four poles or terminations in the course of the current. The points 1 and 2 rested on combined litmus and turmeric paper moistened with Sul. Soda and the points 3 and 4 on other combined litmus and turmeric paper similarly moistened; the two portions of paper were supported on a glass plate so as to be insulated from the table and each other except by the platina wires. On working the machine a few turns,

* [73]



the litmus under 1 and 3 was reddened by acid and the turmeric under 2 and 4 also reddened by the alkali.

74. Then let the positive *a* rest on litmus paper moistened by the sul. soda and touched the paper itself with moist string held in the hand, so as in fact to have a positive pole active but no definite decomposing negative pole near; still the decomposing effect took place there as well as if a negative pole had been active.

75. In this case what becomes of the alkali repelled from the positive pole? Repeat the expt., discharging by water into the air.

76. As identity of common and voltaic electricity proved, we may reason from the former when intense, etc. etc. as to manner of action of the latter.

77. Some water was boiled for 6 or 7 hours until above one half or 2 quarts had boiled away. The [portion that] remained was poured hot into glass retort[s] until they were full to the end of the neck, and then these were immersed in water so as to be cooled rapidly and then the Wollaston's globe and points ( ) filled rapidly with it. In this, water containing no sensible portion of air was experimented with.

78. On passing the electricity through in currents or in sparks there were exactly the same appearance[s] as before. Hence the gas is not air in the water but decomposed water. All the effects looked like decomposition from explosion and not electro chemical decomposition, except indeed when the wires were in contact one with another in every part, and then a minute stream of gas rose from the left hand or negative surface whilst only a bubble rose now and then from the positive. On turning the globe round so as to change the surfaces relative to Pos. and Neg. still the negative gave the minute stream and the positive the little occasional bubbles. Still these effects were not quite constant and I suspect the whole is due to decomposition by explosion.

SEPTR. 4TH.

79. Put the positive point *a* (47) connected with the electrical machine on litmus paper moistened with solution of sulphate of Soda, and then connecting one end of a wet string with the litmus paper, the other end was allowed to hang in the air, the middle of the string being supported on insulating wax rods. On working

the machine, the electricity passed through the wire point to the litmus paper and then away by the wet string, and issued into the air in brushes and streams as from a point, and was dissipated; so that there was but *one metallic pole*, the positive *a*. Yet decomposition took place, for the litmus was reddened there freely as before.

80. Then put a point on to the conductor of the machine; put a piece of turmeric paper moistened in Sul. Soda on to a glass plate to insulate it. Hung a piece of wet string from it; brought a fine platina point (47) into contact with the turmeric paper, holding the wire of it in contact with the discharging train; and then worked the machine. The electricity now passed from the point on the machine, through the air on to the wet string, from it to the paper and away by the point *a* to the earth; so that here also there was but *one decomposing metallic pole* and that was negative. Here also decomposition took place, for the turmeric paper was quickly reddened.

81. Do not these cases shew that *two* decomposing metallic poles acting *simultaneously* are not necessary; and also that each pole has power of attracting one set of bodies and repelling another without the aid of the contrary pole? But where does the repelled element or body go to in these cases? Try by iodine or Hydriodate.

82. Repeated the reddening of litmus paper by sparks as before (61).

83. Then took a piece of litmus paper, cut it into two, dipped one piece in a weak solution of alkali (strong enough faintly to redden turmeric paper); took part of this moistened sample and dried it; took another part about this size  and putting the wires *a*, *a* at each end sent electric sparks over it until so much acid had been formed as was enough to redden the litmus paper besides neutralizing the alkali. Now expected to find it touch paper from the nitrate of potassa formed. But in fact all the three samples of litmus paper (i.e. original, alkalized and neutralized) burnt as touch paper and with sensibly equal power.

84. On putting a piece of the spark neutralized paper into a drop of M. A. and comparing it with a similar piece of the alkalized part similarly treated, the first became nearly bleached, the other

merely red; but this not enough, for the electric sparks might have done something in destroying colour.

85. On putting the spark neutralized paper into strong S. A. in a tube no red fumes; but then a little bit of paper dipped in solution of nitre produced in S. A. *no red* fumes, the carbon of the paper, etc. etc. apparently taking oxygen enough under the circumstances to prevent formation of nitrous acid.

86. Cavendish expt. is sufficient to shew what takes place when the spark is taken in air over a solution of potash, and to prove that N. A. is formed.

87. Litmus paper with much alkali is not good to shew reddening effect of spark, etc. etc., due to acid formed from air, except the experiment is continued longer.

88. Made a few expts. in repetition of those of Wollaston on oxidation of the Amalgam. Rubbed a glass tube with new clean black silk and examined state of tube by electrometer. Some electricity but not much. On putting a little tallow on the silk then abundance of electricity.

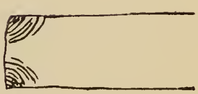
89. Put clean amalgam of platina on clean silk and rubbed the tube; very little electricity. But adding a little tallow, then plenty of electricity.

90. The ordinary amalgam with tallow, however, beat the platina amalgam with tallow.

91. Still I see no proof of the truth of Dr. Wollaston's statement considered in the unreserved way in which he makes it. Nor any that oxidation is the source of electricity in the machine.

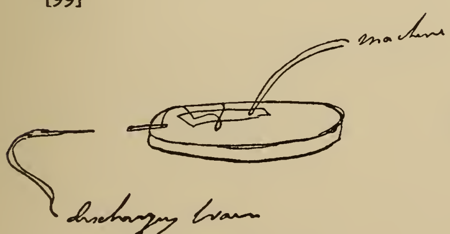
92. The following points are probably connected with and to be considered in conjunction with the attractions and repulsions producing decomposition at an electro chemical pole. *Davy's heaps* in mercury. These must be tried for in strong saline solutions containing particles, etc. etc.; in water also. *Davy and Herschell.* Mercury under liquid bad conductors—do same by common electricity. Ampère's Elongation of current. Decomposition and divergence by water battery—this may perhaps become a good instrument of comparison between common and voltaic apparatus. Brande's expt. on decomposing water in close vessels.

93. Made a few more experiments with amalgams, etc. etc. (88).
94. Black silk will excite glass rod pretty well: if a little tallow be added, very well.
95. Amalgam of Platina on black silk did not excite tube well, but yet quite as well as an amalgam of tin without tallow. With tallow both did far better than before and that of tin better than that of platina.
96. The cap of the instrument (electrometer) was covered with tin foil, and then being flapped with flannel the instrument diverged. When the cap was covered with platina foil, it however diverged just as well. The brass cap of the instrument did better than either, but it may have been lacquered or perhaps the coat of oxide may be effective beyond metal.
97. Glass is easily excited by flannel.
98. All these results are against Dr. Wollaston's notion.
99*. Now experimented in continuation of the former experiments with one pole (79, 80, 81) to ascertain where the element which *receeds* from the pole used goes to and what direction it takes. A little bit of turmeric paper about this size was moistened in solution of sul. soda and put on the edge of a glass plate; then the connection of the positive point *a* (47) was made with the end of the paper on the glass, whilst a point terminating the discharging train was brought about 2 inches from but opposite the other end of the turmeric paper. The machine was then worked, and after 40 or 50 turns it was found that the two points of the outer end of the turmeric paper were deeply tinged brown by alkali, and deepest at the very corners where the electricity had gone off in streams.



100†. Then reversed the order, i.e. a piece of litmus paper was moistened in sol. sul. soda and one end connected with the discharging train, whilst the other was placed opposite a point connected with the machine. On working the machine only a few turns, the end opposite the point became red from acid, the redness being at the two corners where the pos. electricity entered from the air.
101. Then a long piece of turmeric paper, large at one end and pointed at the other, was dipped in the sul. soda and the large end connected with the machine, whilst the small end was held

* [99]



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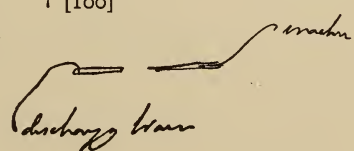

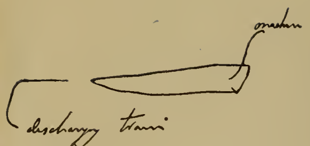

opposite a point connected with the discharging train. After 40 or 50 turns of the machine the point of the paper was reddened by alkali.

102. Then a similar piece of paper was connected with the machine at the larger end and the point directed into the air, *no receiving point* being opposite. After some turns the salt was found decomposed and the alkali collected at the point discharging electricity into the air.

103. Hence it would seem that it is not a mere repulsion of the alkali and attraction of the acid by the positive pole, etc. etc. etc., but that as the current of electricity passes, whether by metallic poles or not, the elementary particles arrange themselves and that the alkali goes as far as it can with the current in one direction and the acid in the other. The metallic poles used appear to be mere terminations of the decomposable substance.

104. The effects of decomposition would seem rather to depend upon a relief of the chemical affinity in one direction and an exaltation of it in the other, rather than to direct attractions and repulsions from the poles, etc. etc.

SEPT. 7TH.

105. As upper atmosphere is generally pos ?, there is almost always a natural current of electricity from above downwards. As this passes through leaves, plants and other things on the surface of the earth, it must have a corresponding effect in modifying the tendency of certain proximate elements this way or that and perhaps may be important in giving direction to plants, the development of the ends of branches, stems, etc. etc. Consider this and try if plant[s] grow differently under conductor or not.

106. Query the influence of water in Electro chemical decompositions, its necessity or not; Brande, Cumming, Ritchie, Davy, etc. etc.

107. If important, then consider the action of spongy platina on oxygen and hydrogen in connexion with the subject and try influence of electric states given to the gases purposely, either when mixed or before they issue from apertures. Effects of points on common machine in such mixed gases. Effects of spongy platina made previously positive or negative, etc. etc. etc.

108*. A piece of turmeric paper *a* and a piece of litmus paper *b* were dipped in solution of sulphate of soda, put together so as to form one moist pointed conductor, and supported on wax between two needle points, one attached to the conductor by a wire and the other to the discharging train. The points were about ½ an inch from the points of the paper; that attached to the conductor was opposite the litmus paper, the other belonging to the discharging train was opposite the turmeric. Hence electricity went through the air, in at the litmus paper and out at the turmeric point. On working the machine a little while, the point of *b* (litmus) became red from acid; the point of *a* (turmeric) red from alkali. There were no poles in contact with the solution except indeed air, yet decomposition occurred. The acid and alkali were arranged exactly the reverse of what would have happened at the end of metallic conductors and appear rather to have gone *with* the current than to have been attracted by any particular surface.

108½. References, etc. to papers connected with this subject.

Wollaston on Chem. Action of Common Electricity. Phil. Trans., 1801, p. 427.

Colladon on deviation of Mag. Need. by Do. Ann. de Chimie, 1826, xxxiii, p. 62.

Davy on the relation between electric and chemical Action. Phil. Trans., 1826, 1821, p. 435.

Herschell on the motion of mercury under fluid, etc. Phil. Trans., 1824.

Serullas remarks on do. do. Ann. de Chimie, 1827, xxxiv, p. 19.

— on motion of mercury, etc. Journal de Physique, etc., xci, p. 170, and xciii, p. 115.

Ritchie on the—

Davy's elevations.

Ampère. Elongation of current.

Brande's decomp. of water in close vessel.

Savary on Magnetization (hist. part also useful for reference) Ann. de Chimie, xxxiv, 1827, p. 5.

Brande on attraction of acid, Alkalis, etc. by Electricity. Phil. Trans., 1814.

Necessity of Water. Brande, Manual, i, p. 88. Davy, Do., i, 97.

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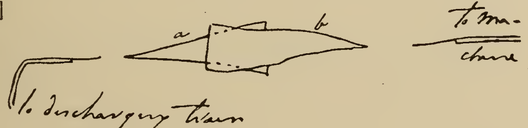


Davy on Torpedo, 1829, thinks it particular electricity. Phil. Trans.

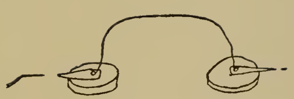
Walsh on Torpedo. Phil. Trans., 1773, p. 461; 1774, p. 464.
Hunter on Do. „ „ 1773, p. 481.
Ingenhousz Do. „ „ 1775, p. 1.
Cavendish Do. „ „ 1776, p. 196.
Todd Do. „ „ 1816, p. 120; 1817, p. 32.

Nobili on new class of Electrochem. phenomena (metallochrome). Ann. de Chimie, xxxiv, 1827, p. 288.
Also Ibid, xxxiv, 1827, p. 419.

109. Then made a similar arrangement, but put the litmus towards the receiving point and the turmeric towards the delivering point. Now no reddening of either happened, so that it was not from the mere effect of the brush, etc. etc.

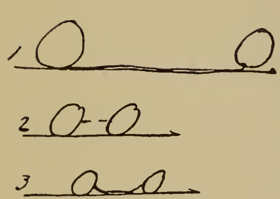
110. Might put several in succession.

111. Then arranged the first order (108), but instead of putting the litmus and turmeric papers in contact, put them on separate glass plates insulated and communicated them by means of a piece of string six feet long, moistened in the same solution of sul. soda. On working the machine the litmus point was reddened by acid and the turmeric point by alkali, but the effect at the latter was weaker than before, for in fact much of the electricity left the string at various parts over its surface.



112. Paper moistened in sol. hydriodate potassa is reddened by brush on to it either from Pos. or negative conductor. But I believe this is because of the formation of little N. A. by the brush in air and its action on superficies of hydriodate. Very little vapour of nitric acid, if strong, browns this salt, but if the N. A. be diluted a little then *much* of it will not decompose the hydriodate. Now when formed by the Spark or brush the N. A. is at the moment very concentrated.

113. A spark taken over the moistened hyd. pot. paper always browns it if the spark be near enough to pass in its course in contact with the paper. But if the spark pass through the air as at 2, then the paper is not discoloured. If it pass down in the middle of its course as at 3—and it can easily be made to do so when the paper lays on glass, etc. etc.—then the part is discoloured only where the spark touched it. If the spark enters into and out of the paper



luminously but is *conducted* within the moisture in the middle part, the discolouring does not take place at the latter part but does where the sparks or brushes were visible as they entered the paper. All these effects seem to prove that it is the acid formed by the explosion that does these things and in fact paper moistened in hyd. potassa is a better test for the acid formed than litmus paper.

SEPT. 10TH.

114*. If two troughs be arranged so that the N. end of one and the P. end of the other be connected by a wire, and a mag. needle placed near the wire, that needle will not be affected (permanently) by the wire unless the other P. and N. ends are also connected. If arrangement and no current, then the first wire ought to be arranged perhaps to half the extent of what it should be when the circuit is complete. But it possesses *no power* at all. This against those who oppose current.

115†. If two troughs be insulated, the N. end of one and the P. end of the other furnished with wires, and a magnetic needle be placed under one of these wires, it will not be affected, but on bringing the two ends of the wires in contact the needle will be affected for a moment; and then when the two troughs are in their natural relation to each other the effect will cease. This again is proof in favour of current when circuit completed and against mere arrangement.

116. By current I mean any thing progressive, whether it be a fluid of Electricity or vibrations or generally progressive forces. By arrangement I understand a local arrangement of particles or fluids or forces, not progressive.

117. Again, decomposition of water infinitely better in a saline solution than in pure water, i.e. best when best conductor used. This natural if a current, for a current may be retarded or accellerated. Ought it not to be the contrary if effect depended on arrangement?

118. With regard to nature of the power which urges this or that element towards this or that pole, can a simple element be so urged or must it be in relation (by combination) to some other element? If effect depends upon relief of attractive tension in one

* [114]

† [115]

direction and increase in the other, a single element would not go to poles (or be attracted as it is said).

119. Or would mere mixtures show effects? Thus oxygen and Hydrogen gases mixed—or carbon in water—or sulphur in water—or metal diffused, as gold.

120. Solution is attraction, and therefore bodies in solution, as iodine in water, or acids only, etc. etc. would not do to illustrate the point.

121. Query the nature and value of Mr Brande's experiments on attraction of bodies by common electricity at a distance.

SEPT. 14TH.

122. Experimented on effects of tension, whole quantity being the same on the deflection of the magnetic needle, thus. Our electric battery contains fifteen large jars of equal size and each with about square inches of doubly coated surface (i.e. inches inside and inches outside). Seven of these were disunited and eight left connected. Then the machine, having been brought into good order, it was connected with the battery and the battery with the galvanometer, so as to send its discharge both through it and wet string in the manner before described (11, 20, 21); the wet string connection was about 10 inches long, quadruple and thick.

123. On working the machine the battery charged up to the 38th turn but discharged spontaneously at the 39th. Then charged the battery with 30 turns and discharged it through the string and galvanometer. The needle was deflected, passing to the left to the $2\frac{1}{2}$ and then swinging to the right to the 3 division or line of an arbitrary and irregular scale of large degrees placed under it. The marks were, however, quite sufficient to determine what extent the vibration occupied.

124. Then connected the other seven jars and charged the whole fifteen with 30 turns of the machine. The Henly's electrometer stood now not quite half so high as before; but on passing this charge through the string and galvanometer, the needle of the latter swung to the left to the $2\frac{1}{2}$ and then back to the right to the 3 division exactly as before.

125. Repeated this experiment with exactly the same result.

126. Hence it would seem that if the same absolute quantity of

electricity pass through the instrument, whatever may be its intensity, the deflecting force is the same.

127. Then sent 60 turns of the machine into the battery and passed the charge through galvanometer. The deflection much larger than before but the graduation not nice enough to tell the proportion; it appeared to be of about twice the extent.

128. Have mounted an aerial apparatus for the collection of atmospheric electricity, but it did not shew any signs on this day, although the insulation very good, as was shewn by its holding a charge given from excited wax.

129. Now drew some platina and zinc wire through the same hole in a draw plate—it rendered them very nearly $\frac{1}{18}$ of an inch in diameter—fastened these to a piece of wood so that their lower ends projected beyond the wood and were parallel to each other and 5/16 of an inch apart. These are to be immersed in acid to constitute a standard voltaic arrangement. The other ends were directly connected with the ends of the galvanometer wire and bound round with clean copper wire to make the contact good.

130. Put one drop of Sulphuric acid into 4 oz. of water, mixed well, then brought it under the ends of plat. and zinc wire, lifted it up until the wires were immersed about $\frac{5}{8}$ of an inch and instantly took it away again. The galvanometer needle swung to the right hand beyond the 3rd division. So that in that moment the two wires in such weak acid produced as much electricity as the battery contained after 30 turns of the machine.

131. The needle being again at rest, the acid was again carried up and the wires left immersed $\frac{5}{8}$ of an inch; the needle swung to the right hand to the 6th division, and settled after vibrating at the 3rd division.

132. A piece of string was dipped in strong M. A. and then brought up for a moment against the ends of the platina and zinc wires. The needle went to the right to the 6th or 7th division.

133. Then separated the galvanometer and the voltaic arrangement and made the upper end of the latter fast to two fine platina wires, and brought them in contact with paper moistened in the usual solution of hydriodate of potassa, whilst the lower ends were immersed $\frac{5}{8}$ of inch in the former acid (130); caused decomposition and shewed iodine at one pole but not at all in proportion to the

deflecting power of same arrangement, for in fact the moistened paper, being a bad conductor compared to the galvanometer wire, allowed only a small proportion of the electricity to pass.

134. But then when the hydriodate potash paper was put flat between two platina spatulas and these touched by the platina points of the galvanic arrangement, then decomposition took place over an extended surface, perhaps more than a hundred times larger than that used with the Electrical machine (46, 52). This natural, for now conduction across hyd. paper facilitated by extent of surface of contact, more electricity passed and more decomposition followed. Now with the machine *all the electricity* passes however small the surface of contact; no retardation (to interference) can take place.

135. Procured a thick platina wire $\frac{1}{12}$ of an inch in diameter, filed one end flat so as to expose a circle $\frac{1}{12}$ of inch in diameter for contact. Then connected a platina spatula with the discharging train, arranged it horizontally, put on it filtering paper of four thicknesses and moistened in the hydriodate of potash, put the thick platina wire upright with its flat end on the paper and pressing with its own weight, and connected the Electrical machine with the other. 30 turns of the machine made a fine deep brown spot $\frac{1}{12}$ of inch in diameter; the iodine even penetrated the second thickness of paper—20 turns of the machine made a paler but still good spot—10 turns gave a pale iodine spot, not sensible in the second thickness. This a very good expt.

136. On using the same decomposition arrangement with the elementary standard battery (129), excited by the acid consisting of 1 drop in 4 ounces water, it produced decomposition but took some time to equal the effect of the machine—time equal I think to that required to turn the machine. But then because of the intervening four thicknesses of paper and moisture the electricity of the voltaic arrangement would be much retarded ( ), that of the machine not being so.

137. On including the galvanometer in the circuit so as to give as it were a measure of the quantity of electricity passing, it took a pretty strong acid and immersion of at least an inch of both wires before the needle was permanently deflected to the 3rd division on the right hand, and even then a momentary contact would not

cause decomposition to the extent of 30 turns of the machine; it required 2 or 3 seconds for the purpose.

SEPT. 15TH.

138. Resumed the experiments of yesterday on the comparitive effects of the Voltaic pile and the Electrical machine.
139. First brought the machine into equally good order.
140. Then arranged the two wires platina and zinc $\frac{1}{18}$ of inch in diameter as they were yesterday at $\frac{5}{16}$ of an inch apart.
141. Ascertained the number of beats of my watch for the swing of the galvanometer needle either from right to left or from left to right, i.e. in one direction it was very nearly 17 beats.
142. Now purposed to immerse the wires of the voltaic arrangement to such a depth, and in acid of such a strength, as to swing the needle as far to the right or the left as was yesterday done by the battery, the time during which the wires were to be immersed being about a half of that required for a whole swing.
143. Used the acid made by putting one drop oil of vitriol into 4 ounces of water. On immersing the wires $\frac{5}{8}$ of an inch deep in this acid and holding them there during eight beats of the watch, after which they were quickly removed, such impetus was given to the needle that it passed to the right hand to the third mark or division, i.e. exactly to the place to which the Electrical battery charged by 30 turns of the machine carried it yesterday.
144. The needle continued to swing towards the 3rd division after the wires were withdrawn from the acid, i.e. the time of immersion was not so long (indeed only about a half) as the time of the first impulse to the right. Hence the whole deflecting power of the current during the 8 beats may be considered as fairly exerted on the needle.
145. This observation was not made upon the first immersion of the wires in the acid, but after they had been immersed two or three times and when the first excessive effect was over. It was repeated several times and always produced the same effect.
146. Hence as an approximation, two wires, one of platina one of zinc, each $\frac{1}{18}$ of an inch in diameter, when immersed each $\frac{5}{8}$ of an inch into an acid consisting of 1 drop oil of vitriol and 4 oz. of water, being also $\frac{5}{16}$ of an inch apart, give as much

electricity in 8 beats of my watch as the Electrical battery charged with 30 turns of the large machine in excellent action.

147. Now my watch gives 150 beats in a minute.

148. Thirty turns of the machine occupy on an average 60 beats. Ten turns gave 100 sparks of an inch each in length from the prime conductor, which consists of two large long brass cylinders and a connecting brass rod. The Plate is 50 inches in diameter, in high excitation, and will produce so much electricity that sparks from 10 to 12 and 14 inches long may be drawn from the conductor.

149. The jars of the battery used yesterday and to-day and at other times are each coated 8 inches up from the bottom and are 23 inches in circumference, so that each contains 184 square inches of glass coated on both sides, independent of the bottom which contains 50 square inches besides.

150. When the wires were permanently retained in the acid (143), the permanent deflection was found to be to the third division on the right hand. Therefore a permanent deflection to the right hand to the 3rd division will indicate a constant voltaic current which in eight beats supplies as much electricity as the battery charged as yesterday with 30 turns of the machine.

151. Now tried some other forms of intensity of Electrical current to accompany those of yesterday (122, 123, 124). The battery, galvanometer and machine were connected so that the discharge would take place through wet string as in the early experiments (11,). The battery was then charged by 50 turns of the machine, discharged through the galvanometer, etc. etc. and the deflection noted. Then the string, which was fourfold thick, short and moistened in dilute acid, was replaced by a thin single long string moistened only in water. The battery was again charged by 50 turns; again discharged by the string, etc. etc., and a deflection exactly the same as before produced. Then the fourfold thick string was used with 50 turns of the Machine and the battery, and then the thin string again, but deflection was always the same. These expts. are in fact the same as 20, 21 and 22.

152. The thin string was 38 inches long. The thick string was full thrice the thickness of the thin string, and as four thicknesses were used each 12 inches long, it is as if a string 38 inches long

was compared with one twelve times as thick and not one third its length. Yet still effect the same, though time and therefore specific intensity of the currents very different, but whole quantity was the same.

153. The difference of intensity was also shewn in way the discharge passed, with the thick string in one dull suppressed discharge, but with the thin string more between a spark and a brush, requiring sensible time. In both cases the discharger, when brought up to conductor, was held there a moment that all the electricity might go through.

154. The thin string gave I think on the whole a little advantage, and if there is any lateral transmission from coil to coil through the silk, it ought to be so, because then intensity lower and lateral transmission less. But the difference was very slight.

155. The term Galvanometer now exceptionable but very useful.

156. Now arranged the decomposing apparatus of yesterday (135, 137) as at 137, i.e. connected the platina wire with the platina wire of the standard voltaic pair (129,), the other or zinc wire of the voltaic pair to one end of the galvanometer wire, the other end of the galvanometer wire to a platina spatula, and then interposed four thicknesses of filtering paper moistened in the standard solution of hydriodate of potassa to a certain degree. Thus current had to pass through all and could be measured by the galvanometer.

157. Now prepared a strong nitric acid (diluted sufficiently) and immersed the ends of the voltaic arrangement until the galvanometer needle stood steadily at the 3rd division on the right hand (150,); the end of the platina wire for decomposition resting on the upper surface of the four fold paper exactly as it would do in cases of decomposition. This strength of current being obtained, now put the end of the decomposing wire (135) on a fresh place of the paper and let the current pass for eight beats of the watch (143), and then took up the wire. Repeated this in a fresh place, and then in another place, and so on until many brown spots of iodine corresponding to the end of the wire were obtained, each due to a current of eight beats.

158. Then arranged the same decomposing wire, paper, spatula, etc. in contact with the Electrical machine and discharging train

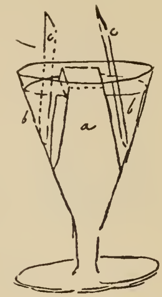
(135), and sent the electricity of 30 turns of the machine through; it produced a spot of iodine as nearly as could be ascertained of the same tint as the voltaic battery. Tried it in another place and another and another and the comparitive effect was very exact. Twenty five turns were not enough, i.e. they produced a paler tint.
159. Hence it would appear that both in magnetic deflection and in chemical effect the current of the standard voltaic battery for eight beats of the watch was equal to the electricity of 30 turns of the machine, and that therefore common and voltaic electricity are alike in all respects.

SEPT. 18TH.

160. Has Porrett's experiment or has Dutrochet's endosmose (Ann. de Chimie, April, 1832) any connection with the passage of elements between the poles in my view of the matter.

1832. OCTR. 26.

161. Have prepared some tremulous jelly about as strong as for table use. One portion with distilled water, which shall be called pure jelly—another with sulphate of soda, which shall be called Sul. soda jelly; there was not enough sul. soda present as to crystallize in the jelly when cold, nor to make it loose of its stiffness as an over dose would do.
162. A slice of the sul. soda jelly put between the poles of a single voltaic trough of 10 plates 4 inches square instantly gave alkali at the negative pole and acid at the positive pole, rendered evident by pieces of litmus and turmeric paper there, and apparently as freely as if the jelly had been liquid. The slice was about $\frac{1}{4}$ inch thick. Decomposition ready therefore and easy.
163. The piece of jelly between the poles did not melt or seem inclined to fuze by any power, but it was pushed about and broken by the gas evolved at the place of contact. A *smell* arose from it *very like indeed* to that of the common electrical machine.
164. The pure or unsalted jelly gave a little acid and alkali at the poles when treated in a similar way, and that not only when the test papers were put in the first instance between it and the poles, but also when the jelly alone was placed between them and its sides afterwards brought into contact with the test papers.



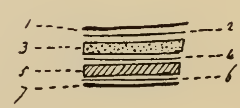
165. A conical glass had been filled with the sul. soda jelly and allowed to gelatinize. Then two portions were cut out so as, by leaving a portion as a wall in the middle (*a*), to form two cells (*b*, *b*) on each side of it. These were filled with strong solution of sul. soda in water and then the poles (*c*, *c*) of the trough immersed in them. There was immediately strong action—much gas evolved—and in a few minutes or even less than a minute the solution in the negative cell was strongly alkaline *throughout* and that in the positive cell acid *throughout*. Acid and alkali had mutually traversed the jelly, yet there was no perceptible acidity and alkalinity in it near the cells but in its mass.

166. The above experiment repeated except that the jelly taken out was melted and then put into cells so that the fluid and separating wall were of same composition in all things, differing only in solid and fluid states. When poles immersed then decomposition, etc. etc. as before, and the acid evolved in one cell exactly neutralized the acid [? alkali] in the other.

167. Then warmed all the jelly enough to fuze it and put it into he glass and immersed the poles as before. The quantity of gas, etc. was such as by the eye to indicate that the action was neither more nor less whether the jelly between the poles was fluid or solid. The transfer, etc. etc. seemed equally easy.

168. Now experiments were made with the cold gelatinous saline and unsaline jelly (   ) by superimposing slices and putting the heaps between the voltaic poles (always platina). The slices were about $\frac{1}{4}$ of an inch thick and the diagrams represent vertical sections of the arrangements.

169. Made a heap with—1, Pos. pole—2, litmus paper just wetted so as not to run with distilled water—3, unsaline jelly—4, litmus paper wetted as before—5, Sul. soda jelly—6, Turmeric paper wetted, etc.—7, unsaline jelly—8, turmeric paper wetted, etc.—9, Negative pole. Contact for a few seconds with the battery evolved plenty of acid at 2 and alkali at 8. But there were no signs of acid at 4 or of alkali at 6. Hence the transference was good, ready and effective, but the evolution seemed to be altogether at the poles. No signs of the free acid or alkali in its transit appeared.



170. Then arranged a heap thus: 1, Pos. pole—2, litmus—3, pure jelly—4, litmus—5, Sul. soda jelly—6, turmeric—7, Neg. pole. By

contact with battery the alkali instantly appeared at 6. No acid at 4, but plenty at 2. The transfer was again free across the pure jelly and to the extremity of the decomposable conductor, i.e. to the metal poles.

171. Then arranged thus: 1, Pos. pole–2, litmus–3, Sul. soda jelly–4, turmeric–5, pure jelly–6, turmeric–7, Neg. pole. On making contact with the battery much acid appeared at 2, no alkali at 4, but plenty at 6. The transfer therefore as ready and to the same extent as before.

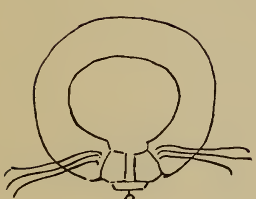
172. Now worked with large electrical machine as at 135, repeating that experiment.

173. Then instead of connecting the discharging train wire directly with the paper moistened with hyd. pot. on which the wire rested, that paper was put on a glass plate and connected with the discharging train by 4 feet of wet string, insulated through its course in the air. The platina wire of $\frac{1}{12}$ diameter resting as before on the paper moistened with the hydriodate solution. On turning the machine 30 times the spot of iodine was as brown as before.

174. Now the hyd. pot. paper was made to touch a piece of paper moistened in pure water and the end of the wire put upon the latter. Upon turning the machine *no spot* of iodine was produced. When the wire was gradually brought on the paper nearer and nearer to that containing the hyd. pot., still no spot of iodine was produced until it actually touched it.

175. Thus the iodine is not transferred instantly and in a free state, but gradually and by successive combination and decomp., and the experiment corresponds with those w[h]ere jelly and the Volt. battery was used in proving the successive combination, etc. etc. of the elements moving between the metallic poles.

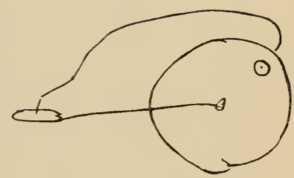
NOVR. 1.



176. Have prepared a large ring electro-magnet. The iron was 1·7 inches in diameter and round. The neutral, i.e. middle circumference of the ring 24 inches. Three coils of copper wire was put round it. The first, or that nearest the iron (linen intervening) was of wire $\frac{1}{8}$ of inch in thickness and 64 feet in length. The

second was of the same wire and 60 feet in length about. The third was $\frac{1}{10}$ of inch in thickness and 70 feet in length, all separated by twine and calico, etc. etc. and covered with girting, etc. etc. The poles were within 0·4 of inch of each other and the faces parallel, so that wheel or plate might revolve between them well. On the outside of the ring they were flattened so as to form two new surfaces to which a lifter could be applied as in the figure above, and thus the magnet be used either for suspension or as acting on copper plate. Its weight altogether was lbs.

177. On connecting the three copper ends at one pole by a cup of mercury and the three copper ends at the other pole in another cup of mercury, and then sending the discharge from my single pair of plates (voltaic) through the three spirals so arranged, the ring became a very powerful magnet. On taking out two of the ends and sending all the charge through only one spiral (the first) the magnetism was not nearly so strong. The voltaic element was a double cylinder of copper and a single intervening cylinder of zinc, the latter inches in diameter and inches in height. The acid was powerful and mixed Sulphuric and nitric.



178. This being rendered a magnet, the copper plate before used was interposed and spun, collectors being applied at the axis and circumference. The rotation was made so as to produce a current through the plate from the center to the circumference. Then the collector at the circumference was terminated by a fine platina wire, the end of which was made to touch a piece of paper moistened in Hydriodate of potassa, which paper was laid on a spatula of platina connected with the axis of the revolving wheel. Positive electricity therefore tended to pass from the plat. point to the paper and through it to the spatula. But after grinding some time not the slightest trace of chemical action could be perceived.

179. Hence the intensity of the current induced by magnetism appeared to be very small; yet they can pass through the tongue, frog, etc. and produce a spark when once formed.

180. *Passing thoughts on other points.* When contact in single circuit is completed there is no spark. When broken there is spark. Is not this presumptive proof that contact is essential to the current and Chemical action not—or that contact is more influential than chem. action.

181. If really spirals of a helix interfere much when not touching and require thicker insulation as Colladon and an American say, then there must be either mutual interference of currents or else more tension in different parts of circuit than is generally imagined.

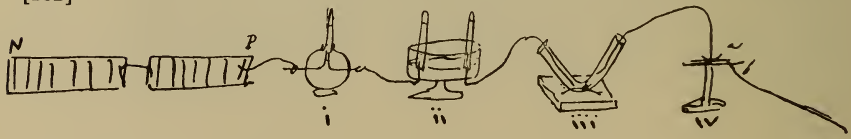
DECR. 15TH, 1832.

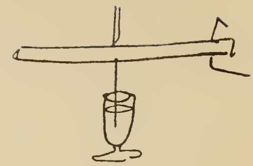
182*. Two trough[s] charge[d] and placed on box as in figure, then connected with our decomposing apparatus, four pieces being used in succession as in the fig. A wire went from P to first apparatus, one from that to second, one from that into third, expanded there into plates, and one from that to fourth into a plat. capsule. i and ii contained sol. sul. soda; iii sul. soda colrd. by cabbage liquor blue; at iv, between the plat. capsule and plat. plate *b* was put two thicknesses of litmus and turmeric paper dip[ped] in some solution of sul. soda. On connecting N with the discharging train and *b* with the earth by a wire inserted only two inches, decomp. took place in *all* the apparatus, gases evolved, acid and alkali set free, etc. etc.
183. On connecting *b* with discharging train and N with earth by two inches wire, same result.
184. On cutting off connection with discharging train but leaving other made, no effect with these apparatus sensible in ¼ hour.

DECR. 17TH, 1832.

185. Threw i, ii, iii and iv out of connection and used merely two thicknesses of paper dipped in hydriodate of potassa as the test between edge of one plat. spatula and plane surface of another. Battery same as before and resting as before on wooden box. When N connected with discharging train and spatula under test paper held in hand only, still sensible decomposition, in fact very fair.
186. When N not connected with discharging train but touched by hand, still decomp., but very feeble.
187. As the box conducts must insulate troughs and then try.

* [182]



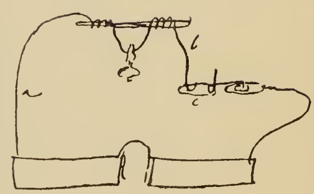


188. Made experiments in various way[s] to obtain if possible a decomposition of water analagous to that of *Dr. Wollaston by wire ends*, using mechanical impulse alone for the purpose. A straight platina wire was fixed to a vibrating bar, and then by a glass rod and wet fingers the bar and wire made to vibrate. The end of the wire was filed flat, but no bubbles or particular effect except that of agitation in the water could be seen at the end of the wire.

189. A narrow tube was put over the end of the wire in the water so as to prevent lateral loss of force, but still no effects.

190*. Then quicker vibrations were used, the wire being fixed at the end of a 6 foot glass rod, fixed at the middle *a* and excited at *b* by a wet piece of woollen cloth. The sounds were high and vibration powerful, but no bubbles appeared. Well boiled water was used in all these experiments.

21 DECR. 1832.



191. Two troughs each 10 pr. Wollaston plates 4 inches sqr. each charged, insulated and connected as one battery. Two platina fine wires were *soldered* on to ends of two copper wires and then fastnd. to a glass rod so that plat. ends were very near together, but not touching. This rod being fixed up, one of the copper wires *a* was connected with the pos. end of battery and the other wire *b* rested on a piece of metal (platina) *c*. A piece of bent platina wire, such as that used 47, connected this metal with paper moistened in standard solution of hydriodate of potassa, supported on a platina spatula connected by a copper wire with the neg. pole of the battery. The whole circuit was thus insulated and decomp. of the hydriodate would have taken place had it not been for the interruption between the points of the plat. wires in the circuit. That there was no contact there was shewn by no decomposition taking place in 10 minutes or a quarter of an hour.

192. But on applying a spirit lamp so as *to heat* the separated ends of the platina wires and the air between, in one minute very sensible decomposition had occurred—shewing that electricity had passed through the hot air.

[1] This date is probably incorrect, and should be between Dec. 17 and Dec. 21.

* [190]

193. On heating the ends as highly as possible by a blow pipe, the decomposition of the hydriodate instantly followed.
194. On allowing all to cool, effects cease. Then voltaic like common electricity can, even with 20 pairs of plates, be discharged through ignited air, etc. etc.
195. On including the galvanometer in the circuit (its vibration requiring 13 beats of my watch) and heating the points for 13 beats, then refraining for other 13 beats and so on alternately, a deflection of the needle was gradually obtained, and decomposition was effected at the hydriodate paper; but this galvanometer was not so sensible to the current as the test paper. Still the effect shewed that the electricity passed through hot air and not through cold air.
196. On putting the ends of the platina wire parallel and very near, think the effects were greater—was more discharging surface.
197. Water battery 1 inch plates charged—1200 pairs of plates will open electrometers about 1/3 of inch—not affect the tongue sensibly—will decompose hydriodate of potassa on paper—will also feebly deflect galvanometer needle.
198. Transferred it to the furnace, put it on glass rods over the sand bath, then rose in power.
199. With two thicknesses filtering paper in standard sol. hydriodate of potassa and standard wire of platina, the whole being insulated and used, 300 beats of watch were required to produce a spot of iodine not quite so strong as that in former experiment 135.
200. Using only the first 100 pair (the warmest) the effect was *better in* 100 *beats* only.
201. Using only the first 10 pair the effect was good but not equal to the 100 pair.
202. 200 pair were not so good as 100 pair—150 pair not quite so good—100 was about the best, but then these 100 were warm, yet not warmer than the first 200.
203. On using the galvanometer as the test of current the effect *improved* even down to ten pairs of plates.
204. These effects in accordance with Ritchie's results.
205. The warm 100 were nearly twice as good as a cold hundred of pairs of plates in decomposing power.

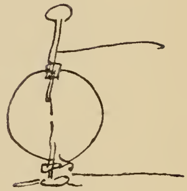
206. A Voltaic battery of 140 four inch Wollaston pairs of plates charged and insulated on glass rods. A person, on standing on floor (stone), could feel a shock if wet fingers grasped the neg. wire—but only a little one if he held pos. pole. Do not know if this due to a little imperfection in insulation.

207. Battery affects the electrometer feebly only—very little.

208. On using apparatus of 191 with wire ends parallel, with hydriodate of potassa on paper in the circuit, there was no decomposition, but on heating wires red hot or white hot then decomposition occurred. On using blow pipe to wires then occurred quickly.

209. With galvanometer obtained deflection as yesterday 195.

210. On using an apparatus of sliding rod in a globe to exhaust, two fine needles being fixed on the lower connection and two on the upper, so as to be brought as near to each other as possible, and connecting this apparatus with the battery poles and decomposing apparatus so as to see if current could pass from point to point through air, could not get any signs of decomposition on paper even though needles brought very near.



211. On exhausting the globe, still could not get any signs of current by decomposition.

212. On using two series of points (9 each) in air opposite to each other, all insulated, etc. etc., still could not obtain signs of current. Still I do think that the electricity of a battery ought to pass from pole to pole by points. I think the intensity is high enough, but whether quantity passed is enough is another question. Points discharge a battery but slowly (26).

213. Now worked with large electrical machine and battery (   ) all in good order. On charging the battery it was discharged thrgh. a wire connected with a platina spatula under the tongue, another spatula over the tongue connected with a wire and about 2 feet of wet thread and the discharging rod. Thus the shock was modified by its passage through water and felt at the tongue.

214. I could take 7, 8 or 10 turns of the machine as a charge; the sensation was just that of zinc and silver over and under the tongue. By putting the one spatula between the upper lip and gum I could easily obtain the flash. I could just perceive the taste in the tongue by a charge of two turns.

215. What a quantity of electricity the pieces of zinc and silver must evolve—again what a quantity even the magnet can evolve at once, as all these experiments shew; and how consistently they group together.

216. I sent the electricity of this machine for a quarter of an hour through an apparatus like that Mr Barry described, but without any sensible effect. 700 turns were given in that time. The machine in excellent action.

24TH DECR. 1832.

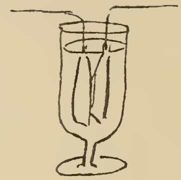
217. Made a thermoelectric circuit with copper and platina wire. On heating the one junction by a lamp, the other being in water, it would deflect the galvanometer and put the needle almost at right angles. Yet interposing one thickness of paper with solution of hydriodate of potassa, I could get no signs of decomposition in 20 minutes. Hence no hope from continuous Magneto-electric current.

218. Can an electric current, voltaic or not, decompose a solid body, ice, etc. etc.

219. If it can, does it give structure at the time.

220. If it cannot, what would fuzed gum, lac, wax, etc. etc. do.

221. De la Rive Chlorine and water—conducting power.



222. Have prepared some platina poles of platina sheet soldered to copper wire, each about 1 inch wide and 3 inches long. Put these in pairs into a glass with a plate of ice between, and again on outside filled all up with powdered ice and ice cold water and put them out last night to freeze. Weather very cold. This morng. all solid, so that poles were in ice facing each other about one inch apart and could be connected by external wires with battery, etc.

223. Used a battery of 20 pr. Wollaston's plates, sent current from it first thrgh. galvanometer and then these poles and the intervening ice. The Needle *was deflected*, and on trying common water with a similar paper [? pair] of poles at similar distance, the needle was also deflected, but not more than in the ice: hence ice conducted as well as water.

224. Paper moistened in hyd. pot. put on the surface of the ice, whilst pos. pole of battery touched the paper and neg. pole one of the enclosed pole[s]. Then iodine slowly apprd. on the paper. Hence current passed ice and decomposed hydriodate.

225. On continuing the battery current thrgh. the ice as in first expt. for 10 minutes or more, the ice thawed at the bottom part and there gas could be seen at the poles in contact with water. But in the middle and upper part of the glass where the ice was in close contact with the platina, there was *no appearance* of fusion nor any of *gas*; no bubbles there. Fusion of ice still continued below, as if current passed there producing heating and other effect, but not where ice in contact with the metal.

226. Took united litmus and turmeric paper, dipped in sol. sul. soda, put it against poles and piece of clear ice between. Order was P. pole, litmus, turmeric, ice, litmus, turmeric, Neg. pole. In this case there was very little deflection of needle, as if current very much retarded. After some time traces of decomposition and acid apprd. at the first litmus paper and alkali at last turmeric paper. But the litmus and turmeric paper *against* the ice were not affected. Is it possible that the acid and alkali could have penetrated the ice? *I think not.*

227. Tried a similar expt. with Hydriodate of potassa. After a

time traces of iodine at positive pole but none against the pos. side of the ice. In these cases ice a little damp: must try more strictly with DRY ICE.

228. Sulphuric acid of 1·783 crystallizes beautifully, but leaves a portion liquid. That is the part not in right proportions.

24 JANY. 1833.

229. Made some excellent expts. on ice. I procured some clean water tight tin vessels open at top, closed below, and then by corks fitted into them the platina poles, so that they did not touch the tin vessels yet the latter served as jackets for them. Then distilled water, which was first boiled for 3 hours, was poured in and instantly frozen by freezing mixtures applied outside. In this way the tin vessel and inclosed pole were separated by a thin layer of pure ice cooled down to 10° or perhaps lower.

230. In the first experiment the tin vessel was 5 inches long, $\frac{10}{8}$ deep and $\frac{4}{8}$ wide. The plat. pole (soldered to a copper wire) was $3\frac{1}{2}$ inches long and $\frac{7}{8}$ wide; the platina was entirely immersed in the water or ice. The voltaic battery consisted of 20 pr. of 4 inch Wollaston's plate[s]. My delicate (first) galvanometer was used. The tin vessel was retained in the freezing mixture during the experiment, and one of the battery wires made to touch the tin case whilst the other was connected with the wire of the enclosed pole. Not the *slightest* deflection of the needle occurred. This was tried again and again. That the connections were all right was shewn by touching the battery wires together; the needle was so knocked round as almost to destroy the galvanometer.

231. On taking out the tin vessel and applying a spirit lamp to the lower part for a few minutes and thawing the ice there, and then again introducing the arrangement into the circuit, the needle instantly spun round and could be retained *permanently* deflected full 60° from its natural position by the current which could pass the distilled water.

232. In a second expt. the pole was a platina spatula $\frac{7}{8}$ of an inch wide and 5 inches long; 4 inches were buried in the ice. The tin case was 5 inches long, $\frac{3}{8}$ wide and $\frac{10}{8}$ deep. When perfectly frozen, it perfectly insulated the electric current; the needle did not move in the least.

233. Then a little water was poured in at top—still the current could not pass. Put a little more in, still not pass, but in a moment or two more it did pass. This was because the water added froze instantly against the very cold metal, and that film insulated the current and even the central portion of added water which did not freeze. When, upon adding a little more water and handling, the ice against the upper edge of tin vessel thawed, and water reached from it to the spatula, then conduction occurred.

234. After the experiment the tin vessel was warmed and the piece of ice drawn out. Then the manner in which the added water had frozen against the metal was beautifully visible. It was also seen that from the slipping of the lower cork the spatula was oblique in the vessel and all but touched at the lower edge on one side. *Yet the insulation was perfect.*

235. A third expt. was made: the pole was platina plate soldered to copper wire. The platina was $3\frac{1}{2}$ inches long and 1 inch wide. The box 5 inches long by $\frac{10}{8}$ deep and $\frac{5}{8}$ wide. At first this seemed to conduct a little electricity for a long while, but it was found that the upper cork dipped into the water, that it was above the level of the freezing mixture and that a little water in it was a long while freezing and continued to serve as a conductor. At last this was frozen and all insulated well and perfectly.

236. Then added water: yet was surprised to find that it would not conduct. Melted the ice in the tin by the fingers at edge and breath, yet would not conduct. Found at last that the cause was the ice was not melted in contact with the *wire of the pole*, for being inclosed so well by the cork, which was embedded in the ice, the thawing process was some time reaching it.

237. Arranged some plat. poles in a bottle of S.A. at S.G. 1·78, which had resulted from draining the bottles frozen before, then fusing the drained crystals and using the acid in the present expt. It conducted very well indeed. But have no assurance that liquid was not present, for in fact it is impossible part should not be liquid, and that part would be at the middle.

238. Electrified an Electrometer and touched it with the wires of the frozen in poles. The ice seemed to discharge electricity of this intensity freely.

239. Have experimented further on insulating power of ice. Arranged the two apparatus 230, 235 as before, but put in so much boiled distilled water as filled only about half the vessel. Nearly two inches of the platina foil was in both cases enclosed in the ice formed by freezing.

240. Used a voltaic battery of 15 trough[s] or 150 pr. of Wollaston plates 4 inches square. It was not strongly charged but it gave an excellent spark with charcoal and a strong shock when hands were moistened. A galvanometer was interposed as before, indeed the same, but when the poles were connected with the tin vessel and the pole frozen in, not the slightest deflection of the needle occurred nor any signs of a current of electricity.

241. Upon removing the vessel and thawing a little of the ice then the current passed.

242. On connecting a common electrical machine in good action with a gold leaf electrometer and touching the cap of the electrometer with the end of the wire connected with the frozen pole, no working of the machine could cause the leaves to diverge. Hence electricity of an intensity so low that it cannot diverge the electrometer still can *pass freely* thrgh. the ice.

243. On trying the battery by the electrometer no divergence could be observed—the electricity was too feeble in intensity to produce this result. Must try by a battery more fully charged and get an intensity which can pass.

244. The expt. 237 was now varied—the wires were so bent that the poles were close to the glass withinside and then freezing mixture was applied to that side first, so as to freeze the acid there and exclude the rest or liquid part to the other side. When the whole was thus well frozen it was put into the circuit. It conducted the current very well.

245. Yet cannot be sure no liquid was there, for substances contract much in cooling, therefore cracks, and there must have been unfreezable portions in some parts of the bottle.

246. Observe next time if any appearance of chem. action.

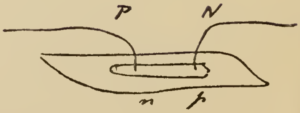
247. Can ice absolutely not conduct voltaic electricity? I think it will conduct high intensity, though perhaps very slowly. Must charge battery till it open Electrometer and then try.

248. If ice will not conduct, is it because it *cannot* decompose?

249. If it will conduct, will it decompose or will the elements separated lodge against each side of the ice?

14TH FEBY.

250. Experimented on substances solid at common temperatures, but fusible and of such composition as was presumed would supply place of or act like water.
251. Two troughs of 10 pr. plates each, double copper and zinc 4 inches square were used, and a galvanometer was introduced into the circuit occasionally to shew if conduction.
252. *Nitre.* A piece of plat. foil was made positive pole and nitre, pure, fuzed on it and allowed to congeal again. A platina wire was made negative pole. Whilst nitre solid it did not conduct, i.e. no current passed thrgh. it affecting the galvanometer. On melting the nitre and then puttting the neg. pole in, the galvanometer needle was *knocked round* as if the metals had touched through the nitre, and strong decomposing action took place with much gas evolved; and the nitre in contact with neg. pole become deep green and flowing off in green streams from it into the rest. On allowing temperature to fall, the moment nitre solidified the current thrgh. it ceased; yet neg. wire actually imbedded and cemented in the nitre.
253. Again when Nitre fluid, if neg. plat. pole was cold no current passed when it touched the nitre, for it solidified a little crust around it which insulated the electricity and it was only when so hot that the fuzed nitre wetted it as water would do, that the current passed.
254. The green part about neg. pole was very alkaline when put into water, and as the green colour disappeared yellowish flocculi appeared there, probably platinum dissolved off by alkali at neg. pole, for plat. in caustic alkali yields green concrete, changing thus in water. At Pos. Pole the nitre quite neutral, not acid—no discoloration there.
255. Now put nitre on foil and fuzed it and put in two wires from battery poles. There were here four effective poles. The end of wire P (positive) evolved gas, plenty, but was not discolrd. and the matter in contact with it was neutral. The part of platina in nitre just below it became a neg. pole (n), evolved no gas,



became green and matter there was alkaline. The part at (*p*) was a positive pole and acted in all respects as the end of P; and the end of wire N was a negative pole acting as (*n*) did.

256. Now fuzed nitre on a watch glass and put in Platina P. and N. poles. At P. pole much gas, oxygen, but as to nitrogen not sure—no discoloration there, no action on platina and residue neutral. At N. Pole, *No gas*, action on platina—dark green alkali formed becoming white in water. The whole after some action was dissolved in water and the flocculi left examined; these in N. M. Acid gave platina, but still a little left which did not go freely in N. M. Acid, and yet yellow like platina; perhaps silica from glass, but same happened with alkali on platina alone—*ought to be examined.*

257. Now fuzed nitre on glass again and used poles of *gold* wire. Plenty of gas at P and no action on metal. No gas at N but *rapid solution of gold wire* there, producing dark brown streams flowing into the rest of nitre and partly disappearing there. Action at N. Pole on gold and Platina is from caustic alkali, probably corroding and dissolving metals.

258. Hence nitre is exactly like water: whilst solid it is a non-conductor and when fluid *a conductor and decomposed*, and therefore water has no particular distinction or action or any exclusive power in voltaic chemical decomposition.

259. *Chlorate of Potassa* fuzed on pos. platina pole. No conduction whilst solid. Conduct and decompose freely when fuzed. At P. pole much gas, oxygen. Am not certain about chlorine: no action on metal there. At N. Pole No gas—Potassium evolved burning in globules there—action on platina there, matter becoming dark green and very alkaline.

260. Hence this salt as Nitre and water.

261. *Chloride of sodium.* When fuzed and allowed to solidify a non-conductor for galvanometer—when fuzed, instantly conducts freely and instantly decomposed. At P. pole free chlorine plenty. At Neg. Pole Globules of sodium—very beautiful. Platinum quickly corroded away there, great discoloration and part alkaline from soda of sodium. Hence this body as water, and like it also a binary compound, only two elements concerned.

262. *Borate of lead on glass*, heated, softened like treacle but did

not conduct either when so softened or when solid. Being put upon plat. foil and heated more highly it at a fair red heat conducted and began to decompose; and at bright red heat was very liquid, conducted excellently and decomposed very rapidly (these effects, however, perhaps not quite equal to those of nitre, chlorate or chloride just described).

263. Much gas (oxygen) escaped from pos. pole or plat. foil below; and at Neg. pole lead was evolved which rapidly alloyed with plat. wire and the latter quickly fuzed away in consequence.

264. Try this with Pos. pole uppermost, plat. foil being negative.

265. Even this therefore like water, etc.; curious to see effects of gradual liquefaction here.

266. *Litharge* on Plat. foil pos. Insulates until melted, then instantly conducts and decomposes. Oxygen from below or pos. pole, and lead at N. Pole rapidly melted platina away.

267. Inverted the order and made lower foil neg. and upper wire pos. Then at P. Pole no fusion of platina or action on it but plenty of gas evolved there. At N. Pole no gas; but plat. foil soon pierced through by alloying with lead and contents run out. *Good.*

268. Hence litharge, like water and chloride sodium, also a binary compd.

269. Prepared an iodide of sulphur, $i\,125 + s\,16 =$ very liquid at moderate temperatures, but did not conduct when liquid. Prepared another with more sulphur. But neither did this either conduct or decompose.

270. Hence these compounds differ from water, etc. etc. in being liquid yet insulating. Yet they contain elements which can go to both poles.

271. The iodine in these bodies continually passes off in fumes and they hardly look like strong compounds. Must try Iodide of Phosphorus, etc. and also iodide of lead. If latter will do, then probably in this way a distinction drawn between ordinary [illegible] and metals in addition to those already known.

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272. Continued the experiments of yesterday.

273. Single equivalents of the carbs. of potassa and soda were

fuzed together. A portion placed on positive platina, fuzed and touched by neg. plat. wire. No conduction whilst solid, but when melted strong action. Plenty of gas at Pos. pole, probably both C. A. and oxygen. At Neg. pole no gas, but metal which burnt and free alkali apparently.

274. Must try this mixture in glass tube.

275. On putting drop of water on plat. foil in place of fuzed substances and completing the communication, action, but not nearly so much as with fuzed substances.

276. On putting drop strong solution sul. soda there, then much more action, and more resembled degree of action on fuzed substances.

277. Now put fuzed sul. soda there; plenty of conduction and decomposition when fuzed, none when solid. Alkali appeared at Neg. pole, no gas. Plenty of gas at pos. pole, but could find no bi sulphate of potash there; could hardly expect it considering the heat. The S. A. probably resolved into its elements or perhaps oxygen and suls. acid.

278. *Sulphuret of antimony* fuzed on plat. foil—did not seem to conduct when solid, but did conduct and appear to be acted on when fuzed. Negative pole quickly alloyed and injured but so also positive pole. Indeed mere fusion of it on platinum acts powerfully on latter. Hence repeat expt. in glass tube.

279. *Glass of antimony* on platina—non conductor when solid—when melted appears distinctly to decompose by pole; but must try in glass tube.

280. Borate of lead repeated. The plat. foil Neg. and the upper wire pole Positive. Much oxygen at Pos. pole, no solution or injury of platina there. At Neg. pole or foil no gas, but rapid alloy of platina with lead and fusion into a hole quickly took place.

281. This general assumption of insulating powers so soon as fluid matter becomes solid a new point, before unsuspected and very extraordinary; seems to confer a new property on the matter in the second state. Curious that as gas and as solid non conduct and that as liquid conduct.

282. This assumption of two states perhaps connected with the conducting power of carbon and non conducting power of diamond.

283. As gases will conduct when hot, why should they not be decomposed by electricity passing through them, either of machine or pile, and thus perhaps act on oxygen, chlorine, Iodine, *mercury*, etc. etc.?

284. The passage of carbon across from one pole to the other, if true, would support this view. Or rather, can it pass and should it pass, and is it not rather Carbonic acid decomposed which deposits carbon on neg. pole?

285. The difference between solid and fluid state shews most important relation and opposition between mechanical aggregation and chem. affinity, i.e. of that affinity modified by voltaic power. First may in some respects be considered even *more powerful* than the latter. Again, is the reverse as to heat, where fluid *does not* conduct and solid *does*.

286. Does not insulation by solid shew that decomposition by V. pile is due to slight power super added upon previous chemical attractive forces of particles when fluid? Since mere fixation of particles prevents it, must be slight.

287. Does it not shew very important relation between the decomposability of such bodies and their conducting power, as if here the electricity were only a transfer of a series of alternations or vibrations and *not* a body transmitted directly. May settle or relate to question of materiality or fluid of Electricity.

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288. Find if possible a solid body which can exist both crystallized and not (? diamond and charcoal), and see if any difference in conducting power in two states.

289. Classes of bodies, not as simple and compound, but as thus:
Conductors both when solid and fluid.
,, when fluid, not when solid, decomposable.
Non-conductors when fluid and solid.

290. Then compare composition of these three classes and the elements present.

291. Examine some of the gases as to non conducting powers? Have doubts of them. Remember approximation of liquid and gaseous state shewn by Cagniard de la Tour's Expts. Hence perhaps decompose dense compound *vapours*.

Continued expts. on fuzed bodies.

292. *Nitrate soda*—easily decompose when fuzed—alkali at Neg. Pole—Gas at Positive, etc. etc.; appears not to conduct when solid.

293. *Hydriodate of potassa*—acts as the rest, decompose, etc. etc.—but by the candle light not quite sure of the iodine at P. P.—must make expt. in a tube. Gas at P. Pole, none at N. Pole. Alkali at N. Pole.

294. *Chloride of lead*—easily fuses. Rapidly acted on—boiling and ebullition when contact made, ceasing when broken—lead reduced at N. Pole and platina quickly fuzed there. At P. Pole dark colour quickly produced as if by sol. of platina—on using a wire for P. P. it was soon dissolved and eaten to a point by the chlorine—no gas evolved scarcely but remaining chloride of lead quickly dark, opaque and even black. Try this in tube also.

295. *Oxide bismuth* fuzed by blow pipe—decomposes by current. At N. Pole no gas but metal and alloy of Platina. At Pos. Pole plenty of gas, probably oxygen.

296. *Caustic potassa* conducts when melted and decomposes I think but phenomena not very marked, not as with Chlo. Potassium, because of water present which decomposes and prevents evolution of oxygen; doubt whether Davy obtained Pm. except *after* decomposing the water—the water began the current, etc. etc. Perhaps worth making in tube.

297. Proto chloride of tin fuzed by lamp on glass and two platina wire poles used—beautiful expt. After water driven off and chloride dry, and fuzing by heat, then easily conduct and decompose. At N. Pole no gas but a globule of soft bright tin formed. At P. Pole plenty of gas, no metal. Gas mixed with fumes of chloride but probably chlorine; try in tube to collect gas.

298. Why with some chlorides, as this, is gas evolved at P. Pole and no corrosion or little, whilst with others, as lead, no gas but immense corrosion of platina there?

299. Query—are not all compounds of single proportionals conductors, or how far does it hold.

300. *Chloride of Silver*—most beautiful—fuzed by lamp on glass—conducts well—intense action—deep descoloration at P. Pole

and apparently chloride platina formed. Metallic silver at N. Pole and on lifting up pole drew out a wire of silver, the metal being reduced as it went on and the communication being completed through it. Might in this way draw out silver bar at rate of $\frac{1}{2}$ inch per second quite regular.

301. Easy with silver poles to go on making chloride at one side and reducing it at the other. Curious case of chlorine travelling along the silver and very well illustrate the nature of the transfer of elements and the seat of action in the parts through which the current passes.

302. Put a piece of the wire in ammonia to remain all night and remove adhering chloride. Will probably give spongy silver.

303. *Iodide of lead* on glass fuzes by spirit lamp. Decomposition by Voltaic current. At P. P. iodine evolved and no corrosion. At N. Pole no iodine or gas or vapour but little globule of soft malleable lead. *Good.* Do well in tube to shew place of iodine.

304. *Chloride of copper* (*rosin of copper*). After fusion on plat. foil decomposed by E. current. Copper metallic and red at N. pole and gas at P. Pole. P. Pole very much brightened by action of chlorine on it.

305. *Calomel.* Evaporates from Plat. foil before it fuzes—no induction during the time.

306. *Chloride Manganese* on plat. foil fuzes by lamp—decomposes by current. An exploding gas about N. Pole I think and when the N. Pole taken out into air, the adhering matter goes on swilling and bubbling for some moments. Must try all this by day light and in a tube.

307. *Chloride of Barium—good.* On plat. foil, fuze by lamp and blow pipe. Then decomposed by E. current. At P. Pole much gas (chlorine) and action on plat. foil. At N. Pole burning of gas and *Barium* there, and when lump of chloride collected there and afterwards put into water, it effervesced, *evolving hydrogen* and giving a solution containing caustic baryta (as well as mur. baryta) and shewing carbonic acid in air. On adding a little Mur. acid to matter at N. Pole evolution of hydrogen more abundant.

308. Make this in tube and obtain chlorine.

309. The reason why with some chlorides the chlorine at the P. P. combines with platina entirely, and with others not, is most

likely because resulting chloride is in first case sol. in the original chloride and in the other not. Its affinity helping or predisposing.

310. *Chloride of Zinc*—fuzed on glass till ebullition ceased and matter began to sublime abundantly. On putting two poles in, abundant decomposition—at P. Pole some gas but great corrosion of platina and dark solution there in consequence—wire quickly melted away. At N. Pole some gas continually, perhaps hydrogen from little water present, yet reduction of zinc there. Took piece at end of N. Pole and washed with water to remove Mur. Zinc—the rest was black and brittle—in M. A. effervesced giving hydrogen and sol. Mur. zinc—doubtful whether platina in it. Heat not high enough to melt the zinc, hence very porous and spongy though metallic.

311. Chloride of zinc is a very deliquescent body.

312. *Chloride of Nickel*: could not fuze it on plat. foil.

313. *Iodide Mercury*. Did not seem to decompose when fuzed but volatilized so fast as to annoy me very much—very injurious and poisonous probably—must try expt. in tube. This was the red Biniodide. Try also prot iodide; that single proportions.

314. It is a very remarkable coincidence that all these bodies should thus by change of form either lose conducting power for heat and gain that for electricity or lose that for electricity whilst they gain that for heat. Probably a natural dependance of the two may here be discovered.

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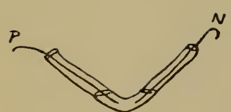
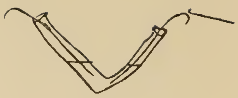


315. *Sulphuret of Antimony* in a green glass tube. When melted conducted Electricity of two troughs freely to galvanometer. When allowed to solidify in tube then did not conduct the least—when warmed and melted conducted again—could not find traces of decomposition during the time the current passed. Would be important to know because would settle whether decomposition and conduction essentially connected in *known* compound bodies or accidentally coincident in some only.

There did not appear to be alloy at N. Side or more action than at P. side. No gas at either—no difference in appearance of poles afterwards when taken out.



316. *Sulphuret of Silver—very extraordinary.* At first on piece of glass flask in air, but afterwards in tube, fuzed into its place in fire.
317. When all was cold conducted a little (by galvanometer) and if quite cold at first conducting power did not increase. But if battery current strong from recent immersion of plates or if sulphuret warmed a little by a lamp—then as contact with poles continued the sulphuret continued to increase in conducting power as shewn by the galvanometer, and at last needle jumped into a position perpendicular to that of the coil as if on a sudden the whole electricity of battery had passed; this then continued as long as contact continued. *The heat rose as the conducting power increased* (a curious fact), no other source of heat than the current being present. Yet I do not think it became high enough to *fuze the sulphuret.* The whole passed whilst in the solid state. The hot sulphuret seems to conduct as a metal would, and could get sparks with wires at the end and a fine spark with charcoal.
318. The sulphuret when hot seems to me very much to resemble the metals in their usual state as to electrical relations, a conductor, solid, not decomposable, etc. etc. etc.
319. If the sulphuret cooled then returned to first state and then all these effects could be obtained again. Hence no permanent change inside.
320. Now opened the tube. On breaking off the glass appearances of silver on outside of sulphuret; these probably from reduction by heat in tube. Tried to trace the platina wires by filing, breaking, etc. etc. to their termination. The ends appeared to be full $\frac{1}{4}$ inch apart.
321. The silver outside appeared more at P. Pole than N. Pole, but doubt its being the result of Voltaic action. If it was, must have resulted from action on the solid body, and thus a new case of voltaic decomposition.
322. Repeat these expts very carefully. Perhaps mixture of Antimony and silver do.
323. Is it not very probable that alloys of Platinum, Silver, etc. etc. with Antimony, Arsenic, etc. etc. would act as sulphurets, i.e. as non conductors though both elements conduct?
324. *Realgar* (sulphuret) and *orpiment* (sesqui sulphuret) when fuzed on glass or when solid did not seem to conduct. Platina

soon melted away, and sulphurets burned off. Hence reason for thinking arsenic will act as sulphur does.

325. *Sulphuret of Bismuth.* Conducts hot or cold, solid or fuzed; may get sparks from a piece almost as a metal. Quite unlike sulphuret of silver. But must try this with some fresh sulphuret made so as to assure the absence of metallic bismuth.

326. *Expt. further with Sulphurets of Silver*, *Bismuth*, *Potassium reduced by hydrogen*, Tin–

327. *Nitrate of Silver*–fuzed on glass–insulates when solid–conducts and is decomposed when melted–furnishes a beautiful case. At P. Pole plenty gas, Oxygen. No acid, red fumes. And at N. Pole no gas but metallic silver arborescing through the liquor and crystallizing in long spiculæ adhering by their points to each other–*very beautiful.*

328. *Bichromate potassa*–fuzed on glass–insulates solid–when melted conducts and decomposes. At P. P. plenty of gas, oxygen, and at N. Pole potash and I think Metallic chromium.

329. *Chromate potash yellow*–requires high heat–fuzed on plat. foil–whilst solid insulates, when fuzed conducts and decomposes, but do not know accurately what and where matters are evolved; think that oxide of chrome is formed; but the general fact of solid and liquid condition good.

330. *Borate of Ammonia* fuzed left only Boracic acid and that insulated at all times. Think it could not be made liquid enough to conduct. See Borate of lead for similar case.

331. *Borax*–on foil, fuzed. Insulated when solid, when fuzed conducted and decomposed. Much gas at P. P. (oxygen?). At N. Pole, sodium; corrosion of plat. wire there, dark colour like Boron produced. On putting matter about N. Pole into water effervescence of hydrogen. Think that Boron had here been separated, but the darkness may be due to action of Sodium or Soda on Plat. Pole, etc. etc.

332. *Chromate of Lead*–fuzed on plat. foil. Then conducted well and decomposed; much action–some corrosion of platina at N. Pole by lead, I believe, and much chromic oxide formed. Did not conduct when solid.

333. Have washed the matter on the Neg. Pole of the Borax Expt. and find it is boron, for it burns into boracic acid with all the characters, etc. etc.

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334. *Spermaceti*: no conduction of Electricity of 2 troughs, tested by a galvanometer, either in solid or liquid state. No decomposition.

335. *Stearine* of cocoa nut oil *Naphthaline* *Camphor* *Shell Lac* *Rosin* *Gum Sandarach* *Adipocire* *Mixed Margaric and Oleic acids* *Caffeine*	As above—did not conduct in either state.

336. *Kino and Catechu* decomposed by heat before properly melted—no results.

337. *Sugar*—when fuzed and so forward to decomp. by heat. If it transmits any current it is so small as to be unimportant—almost a non conductor—and not sensibly decomposed by current.

338. Pyroligneous ether Two ether + 1 alcohol Alcohol Ether	Perhaps fuzed sugar comes with these.

These conducted not quite so well as distilled water, and by same apparatus and trough distilled water very far below (hundreds of times) that with a drop of saline solution; and yet that again far below some of effects with fuzed salts, sulphurets, etc. etc.

339. *Margarate of Lead*. Did not seem to conduct but soon began to decompose by the heat and was then a thick soft paste full of bubbles.

340. *Chameleon Mineral* conducted and decomposed when fuzed but could not tell difference from potassa.

341. *Silicated Potassa*. When melted conducts, etc. etc.; decomposes. Potassium at Neg. Pole. Much gas at P. P. As to *silica*, do not know whether any occurred at N. Pole or not.

342. *Boracic acid*, not quite anhydrous—when fuzed seemed to conduct a little, about as distilled water. Continued boiling all the time because of heat applied and therefore results confuzed.
343. *Acetate of Potash*, fuzed on glass, wires in and salt solid—no conduction. When fuzed conducts freely and decomposes. Potash at N. Pole. Gas at both Poles. No free acid at P. Pole, but it may have been driven off by the heat as separated. Appearance of carbon or black matter there. ? its nature.
344. *Proto sulphate of Mercury*. When solid not conduct. When fuzed then conduct and decompose—much gas evolved, etc. etc. etc.
345. *Mur. Glucina*—did not fuze but sample very impure.
346. *White Arsenic*. Volatizes in open tube ; must be made in a close  tube under pressure.
347. *Glass of Antimony* in tube . When melted conducted pretty well, but not as some sulphurets (silver, hot) or metals or even fuzed salts. When solid wires fuzed in *not conduct* at all. When melted decomposition. At P. Pole could not see if gas evolved. At N. Pole Antimony separated and alloy of Platina formed.
348. *Chloride Magnesium*. When solid and wire fuzed in, non conductor—when fuzed conducted very well and was decomposed. At P. Pole much action and gas—chlorine? At N. Pole Magnesium separated and no gas. Sometimes Magnesium burnt, flying off in globules burning brilliantly. When wire at that pole put in water or dilute M. A. matter round it acted powerfully, evolved hydrogen and forming Magnesia; and when wire and surrounding matter heated in spirit lamp *Magnesium* burnt with intense light into *Magnesia*. VERY GOOD EXPT.

349. *Sul. cyanate of Potassium*. Easily fuzes but decomposes in air or on plat. foil and was at last fuzed in tube. When solid and wires fuzed in, insulated. When melted, instant conduction and decomposition. There was a little gas at P. Pole. None at N. Pole. Both wires remain bright and there is no apparent alteration of matter at either pole. When made hotter substance decomposes and platina becomes sulphuretted at both poles—the results are

then confused. But compound shews the general fact of effect of change of form.

350. Water appears to be the lowest body almost instead of the highest, as respects transmission of elements, etc., etc., etc.

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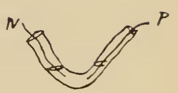
351. Chloride of silver fuzed on glass, using silver poles. All went on beautifully. The positive pole was so rapidly dissolved and converted into chloride that it actually burnt in the surrounding chloride of silver, and at the Neg. Pole the silver was reduced in a wire that could be lifted up and lengthened 5, 6 or 7 inches even. Beautiful.



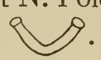
352. Chloride of silver in a tube. Platina poles. There was rapid solution of platina at P. Pole, but still evolution of gas there and that gas was chlorine, *bleaching litmus paper freely*, etc., etc.

353. Chloride of lead in tube. Lead at Neg. Side. Chlorine plenty at Pos. Side. Platina rapidly dissolved and litmus paper bleached.

354. *Chloride Sodium* in tube as above. Sodium and alkali at N. P. (air has access). At P. Pole corrosion of platina rapidly and gas evolved, reddening and bleaching litmus paper.

355. *Iodide* Potassium in tube. Great abundance of iodine at P. P.

356. Iodide of lead in tube—great plenty of iodine at P. P.—decomposition very beautiful. Lead at N. Pole.

357. *Per iodide of mercury*—in tube . When solid in tube, wires being fuzed in, did not conduct—on liquefaction by heat conducts but *does not* appear to decompose. No iodine at P. P.—no appearance of action—no mercury separated—no extra heat to any extent—raised the heat until the iodide boiled but the effects did not change.

358. Cannot procure a protiodide of mercury that will fuze—it resolves itself then into mercury and per-iodide.

359. *Nitre* in tube , fuzed. Abundance of oxygen at P. Pole during action; lighted a spark there, etc., etc. Alkali at N. Pole.

360. *Chloride Manganese* in tube . Metal Manganese at

N. Pole. Chlorine at P. P. Litmus paper bleached there and solution of platina.

361. *Chloride of Magnesium* in tube. Magnesium at N. Pole, Chlorine at P. P.; solution of platina there rapidly and bleaching of litmus paper.

362. *Perchloride of tin.* Not conduct or decompose even up to boiling point.

363. *Protochloride of tin.* Truly conducts and decomposes in tube when melted—not when solid—when decomposing, Metallic tin at N. Pole—but at P. Pole no corrosion of platina or escape of chlorine, but formation of perchloride of tin in abundance. Hence it boiled violently there and evolved fumes, etc. etc.

364. *Chloride of Barium* in tube  ; at N. Pole Barium. At P. Pole acid and chlorine, acid from action of moisture in litmus paper probably.

365. *Chloride of Antimony.* When solid conducts a little, perhaps about as pure water—no signs of decomposition. When fluid by heat conducts better but still not vastly, yet the transition at the moment of liquefaction distinct. Does not then seem to decompose. This is considered as the protochloride of antimony. Query the relation of antimony therefore to other bodies, as sulphur, Phosphorus, etc., etc.

366. *Sulphur* fuzed does not conduct in any state, thick or thin, cold, warm or hot, etc. etc.

367. *Phosphorus*—does not conduct.

368. Alcohol may serve as a vehicle to other bodies though it does not conduct itself. Look for salts and compounds soluble in it.

369. Try all those which seem to be bad conductors by chem. action as a better test than Galvanometer.

APRIL 1.

370. *Cyanide of Potassium*—fuzed on glass—does not conduct when solid—conducts well when fluid—became red hot by conduction. At first no effervescence at either pole, but platina dissolved rapidly at Pos. pole and precipitated in arborescent forms at Neg. pole. By degrees salt or compound became alkaline,

probably from action of the air, and then there was effervescence at Pos. pole. When cold, salt solid; opaque yellow substance from plat. present probably—there was much plat at Neg. pole. Not much indication of potassium at N. pole. Should make experiment in close tube for results. The solution of plat. very curious. If other metals used probably get very interesting formation of new compounds this way.

371. *Fluoride of potassium*—when solid not conduct; when melted conducted well. Effervesced at Pos. pole—apparently little corrosion of platina there but not much. At Neg. pole potassium and alkali. Query, gas evolved at P. Pole worth looking after.

372. *Metallic arsenic*—when solid conducts well as a metal and deflagrates at point of contact.

373. *Chloride of Arsenic*—liquid—does not conduct to galvanometer—nor its liquid hydrate. But when still more water added then conduct and produce gas at N. pole with Arsenic, probably arseniuretted hydrogen, but no gas at P. Pole unless still more water.

374. *Artificial camphor*—solid or fuzed, not conduct to this galvanometer.

375. *Glacial acetic acid*—when liquid not conduct, i.e. no appearance of decomposition and no deflection, but galvanometer not very sensible now. On adding a little water still no signs of conduction. On adding as much water as acid then decompose slowly but not affect the needle. Repeat this and similar results by chem. action and better galvanometer.

376. *Sulphuret of potash* of Pharmacopœia in tube. When melted dull red hot conducts very well to galvanometer; when cooler, though hot, conducts but little. When quite cold conducts not at all. When hot did not conduct enough to sustain sufficient heat without flame. On examining wires, that at N. Pole decidedly more sulphuretted than the other, but potassium there might aid. Whilst conducting electricity there was decomposition there of some kind and effervescence going on in the tube, but I do not know of what substance, this body not being a pure metallic sulphuret, but a mixture of sulphate, etc. etc.

377. *Sulphuret of silver*—very strange. Made some fresh pieces, taking all pains to avoid reduction of it in crucible. Pieces were

from bottoms of crucible, cooled in vapour of sulphur and water and about this size[1]. Filed the face flat to rest on spatula and the cone also filed all over to make contact above. Pieces were arranged as in sketch, all superficial silver being removed by file and sandpaper. They rested [on] platina spatula made Pos. pole and were pressed down by another plat. spatula made Neg. pole.

378. When cold no apparent action generally and yet sometimes needle deflect as if a current; then it would go off again, or come on and increase quite irregularly. If conducts when cold it is but very little indeed.

379. But on applying heat below P. spatula, conduction soon occurred and increased, and heat rose and needle jumped into its place as at former time; and a crackling and vibratory action was heard as if boiling between sulphuret and P. pole, and afterwards sulphuret found fuzed to P. pole. Yet heat from lamp had not been alone enough for that. When once conducting the current sustained the heat high enough to continue the conduction.

380. During action of heat saw feeble combustion of sulphur from part near lower spatula, but am not sure heat alone would not do that. Also appearances of reduction at upper or N. Surface; white spots like silver. But to prevent this or other films of silver interfering with result, put sulphur upon sulphuret so as to melt it there, and could here, where no metallic silver occurred, still conduct and take even a spark when hot.

381. Part on P. spatula also seemed more fusible as well as more combustible than common sulphuret of silver. Must prove unexceptionably whether Sulphur does or not go to P. pole and silver to N. pole.

382. Must make this expt. in vapour of sulphur—also by contact of silver instead of copper—also reverse poles and see if same ebullition, etc. etc. if N. pole made lowest.

383. It seems possible to make a sulphuret with more sulphur. Refuze this *sulphuret* with more sulphuret.

384. All the effect[s] of Electrochem. decomposition seem to shew that in ordinary chem. affinity the particles exert an influence not merely on those with which they are combined but also, although to a weaker extent, upon those particles combined

[1] The diagram is reduced $\frac{3}{5}$ in size.

with their neighbours: that in fact that it is not a mere tendency to unite particle to particle but that tendency is general; and that even those in excess exert an influence though it be not enough to overpower definite combination. Many facts in chemistry also bear out this view that particles act in common. Berthollet, Phillips, etc. etc. have quoted cases. But it is not merely incidental in these phenomena. Electro-chemical decomposition seems to be *essentially dependant* upon it.

APRIL 5TH, 1833.

385. Have been working still on Sulet. Silver to obtain unexceptionable results. First broke some of former artificial sulphuret, mixed it with sulphur and re-fuzed it in a glass tube surrounded by plat. foil. Obtained a good twice-fuzed sulphuret, I think now unexceptionable, i.e. uniform and containing no free silver.

386. Used *four freshly charged troughs*. With small piece of cold sulphuret conducts a little by galvanometer. On squeezing parts, warmth (by fingers, etc.) increased and sulphuret *increased* in conducting power. On applying heat by a lamp, still further increase in conducting power, conducted like a metal, gave sparks both at P. and N. pole. At P. pole reddish, at N. Pole greenish, as of burning silver, i.e. at P. pole where sulphur set free like burning sulphuret.

387. The current of electricity did not with this small piece sustain the heat; it lowered and at last the current itself ceased to pass.

388. Whilst the current passed either moderately or strongly there was voltaic decomposition. Silver appeared at Neg. pole and SULPHUR AT THE POS. POLE.

389. When the current ceased it could be restored by refiling or renewing the contact by a fresh surface at the P. pole. On small masses the course of effects tends to cause interruption to the voltaic current; this is effected at the P. pole and I think perhaps by the intervention of the sulphur separated, or by formation perhaps of a non conducting bisulphuret of silver. The sulphuret is always there altered at point of contact. It has a fuzed

appearance and breaks off from contact with the plat. pole with which before it had adhered.

390. On using silver for Pos. pole then pole *always eaten* into a hole, for sulphuret of silver is formed there. Now also conducting power does not tend to cease as with platina pole, and expt. affords a case of transference of sulphur analagous to that of chlorine in chloride silver.

391. How well decomposition of Sulrt. Silver and place of sulphur shews that the direction of the elements depends upon the other elements with which they are associated, and that the whole effect is an action of the elements *aided* in one direction by the electrical current—and not therefore real cases of attraction by the poles.

392. In all cases silver reduced at N. Pole.

393. The different coloured sparks at two sides of a decomposing body as sulrt. silver may indicate in some other cases decomposition of substances not as yet suspected or separated.

394. On using a larger piece of sulphuret touching by larger surface at Pos. Pole, found that the first current produced heat enough to increase temperature and conducting power and that because of less opportunity for loss, this rose up to full degree, without application of extraneous heat.

395. Found in this case also that conducting power *continued* whilst poles and piece were all pressed together. Not ceasing as with the small piece.

396. Found also that sulphur rose the whole time from the P. Pole, the heat being enough to volatize it and even to prevent probably the formation of bi sulphuret of silver, which interfered in continuance of conduction with smaller pieces. In smaller pieces this could be compensated for by applying the heat of a lamp on purpose.

397. Hence it is quite clear that solid can conduct—that it can *decompose* whilst solid—that increasing heat *increases* conducting power—that elements are electrochemically arranged—that sulphur is either P. or N. (as folk say) at pleasure.

398. In these expts. a little sulphur can often be collected at the P. Pole on the plat. foil—being burnt off in a dark room it is readily visible. But in other cases it does not appear there so as

to be thus detected. This is, not because it has not been separated there, but because heat of action (necessary to conduction) *has volatilized it.* Whilst the current is passing, sulphur can always be smelled burning off or else subliming unaltered at P. Pole, and if the decomposition is made in a dark room it is seen burning off in a pale blue flame round the point of contact of sulphuret and the Platina P. Pole. Hence care in trying to observe the sulphur.

399. Hot platina is to the sulphur at the P. Pole just what Plat. is to oxygen at the P. Pole, i.e. it does not combine with the sulphur but sends it off free.

400. *Native Grey sulphuret of silver*, vitreous—not the flexible or malleable. When cold it conducted a little but did not heat of itself. On being heated conducted much more—ignited at points of contact—sulphur sublimed—arsenic set free, but perhaps by heat only, for white stain and appearance at both poles. There was no appearance of metallic silver at N. Pole. The arsenic going there also probably prevented it.

401. *Red silver ore, Ruby silver—Sulphur, Antimony, Silver, etc. etc.* When cold conducted a little. When warm more; then became of itself red hot and fuzed—because of fusion could not make out results well. At P. Pole plat. foil eaten through. At N. Pole *Plat. Spatula* not apparently much affected, but no distinct trace of silver.

402. When little buttons on P. and N. Pole allowed to cool and then brought in contact they conducted a little; then heat rose, they *conducted more* and soon fuzed.

7TH APRIL 1833.

403. Under certain limitations probably those bodies composed of elements most opposed and having most powerful affinity for each other are those soonest decomposed.

404. Hence also probably a limit or restraint on decomposing power of pile, not because bodies exist by powerful attraction but because not in the relation to each other favourable to these decompositions.

405. There is also a necessity of certain proportions in the number of particles in a decomposable compound. This is shewn in the chlorides.

406. Metals *may* not be compounds of elements most powerfully combined but rather of such as are so similar to each other as to pass out of the limit of voltaic decomposition.
407. Must look for some other bodies which, like sulphuret of silver, will decompose whilst solid. Probably hydriodate of potassa one, and indeed many other bodies tried by common electricity. Probably many such slow actions in nature.
408. Search for a body which, not having yet been decomposed or is not decomposible by electricity, will still insulate when solid and conduct when fluid. I doubt whether there is such body. Per Iodide mercury, 357; chloride of Antimony, 365.

APRIL 13, 1833.

409. Why did Davy require water in decomposing potassa?
410. If decomposition by voltaic battery depended upon the *attraction* of the poles being stronger than that of the particles separated, it would follow that the *weakest electrical* attraction was stronger than the strongest or than very strong *chemical* attraction, i.e. such as exists between oxy. and hy.—Acid and alkali—Pm. and Oxygen—Chlorine and Sodium, etc., etc., etc. This not likely.
411. If voltaic decomposition of the kind I believe, then review all substances upon the new view to see if they may not be decomposable, etc. etc. etc.
412. New view gives good reason why a body should sometimes go to one pole and sometimes to another, according to the element it is in relation or association with. Such as Nitrogen, Sulphur.
413. May not non conductors be decomposed by diminishing their insulating power until the electricity will pass? This perhaps by bringing the poles closer—increasing the powers of the battery —melting or heating the substance. Try sulphur, phosphorus, etc. etc.
414. Volta-electric decomposition may be only one form, and that a very coarse, violent and common form, of what is done upon a similar principle by electric currents. May be innumerable other actions upon similar principles, terminating in rearrangements in particular places of elements moved or affected: these perhaps more quiet but also more beneficial in their results and constantly occurring in nature and animated or vital beings.

415. A single element is never attracted by a pole, i.e. without attraction of other element at other pole. Hence doubt Mr Brande's Expts. on attraction of gases and vapours. Doubt attraction by poles altogether.

APRIL 18TH.

416. Have been decomposing, using as the Pos. pole a platina wire with the end bent into a ring, so that a higher heat could be given to the matter upon it and consequently salts fuzed that could hardly be melted on platina foil.
417. *Chloride of potassium*, insulate solid, conduct and decompose fluid.
418. *Chloride of Calcium*, Do.
419. *Chloride of Strontium*, Do.
420. *Nitrate of baryta*, Do.
421. *Nitrate of lead*—required care or else was decomposed by flame and heat, but still presented same phenomena as the others. Insulate when solid, conduct when fluid.
422. *Carbonate of Potassa*. Do.; i.e. insulate solid, conduct melted.
423. *Carbonate of Soda* Do.
424. *Glass of Phosphorus*, i.e. Acid Phos. lime. Do.
425. *Phosphate of Lead*, Do.
426. *Phosphate of Soda* }
427. *Phosphate of Potassa* } the same; a phosphuret of sodium and potassium formed at Neg. pole, giving phos. hydrogen by action of water.
428. *Boracic Acid*—could not get this to conduct by heat of ordinary blow pipe. Must try oxy-hydrogen blow pipe.

APRIL 19TH, 1833.

429. *Protiodide of tin*, insulate when solid, conduct when fluid and decompose.
430. *Per Iodide of tin*, insulate when solid, insulate when fluid, does not decompose.
431. *Iodide of zinc*, insulate solid, conduct when fluid, decomposes then.
432. *Protoxide of antimony*, insulates when solid, conducts and

decomposes when fluid, about as good as glass of antimony, not better.

433. *Native malleable grey sulphuret of silver*—exactly as the artificial sulphuret of silver—conducting power increased by heat in the same way. Is also decomposed in the same way, silver at negative pole and sulphur at the positive pole.

434. *Native per sulphuret of Iron*—brilliant yellow unalterable—conducts (two troughs) very well—gives sparks—heats very much, far more than metals, etc., etc., evolves fumes at both poles—could not find *certain* traces of decomposition. Is like sulphuret of bismuth as to conducting power.

435. *Arsenical sulphuret of Iron*, native, conducts and becomes heated.

436. *Artificial proto sulphuret of Iron*—conducts well and becomes hot.

437. *Native yellow sulphuret of copper and Iron*—conducts and heats much—it gradually lost conducting power, but I could not find certainly that this was due to decomposition and deposition or intervention of sulphur.

438. *Grey artificial sulphuret of copper*—conducts very well—gives a spark—could find no appearance of decomposition.

439. *Sulphuret of antimony*—little or no conduction hot or cold.

440. *Sulphuret of bismuth, artificial*—conducts well like sulphuret of iron—gives a spark—heats much—decomposition doubtful.

441. In these decompositions of sulphurets, use silver poles and compare effects at two side[s]—this will do when can not separate and collect sulphur.

442. *Grey sulphuret of tin, Artificial*—conducts—heats—does not give spark—does not seem to conduct better for heat—does not appear to decompose but am not quite sure. Must try with re-fuzed sulphuret and silver poles.

443. *Chloride of antimony*—unexceptionable in glass tube  —with two troughs. When solid did not conduct to affect galvanometer—when fluid conducts, but not much—and also decomposes, but only slowly—it is not remarkable for assuming conducting power on liquefaction—but does assume it and decompose.

444. *Phosphate of copper*—when solid non conducts—when fluid conducts and decomposes well. Phosphuret of copper formed at Neg. Pole.
445. *Sulphate of lead*—when solid insulates—when melted conducts and decomposes—effect striking.
446. *Borate of Lead*—when solid insulates—when melted conducts and decomposes—lead reduced at N. Pole.
447. *Per borate of tin.* As borate of lead.
448. *Borax.* When softened conducts a little—when hot and fluid conducts much more—decomposes well—corrosion of platina at the N. Pole—explosion of the borax from formation and combustion of sodium. A brittle hard solid metallic mass was found at the positive pole. Query, is it Boruret of Platina formed there, or is it an alloy of potassium and platina from neg. pole by accident; or what is it?
449. *Nitrate of Strontia.* When solid insulates—when fluid conducts and is decomposed, and during that time the negative pole is covered with a very fine pure blue light—no appearance of red or crimson in it.
450. *Nitrate of copper*—indications of similar action with the former salts, but not certain—at first conduction, and by degrees it loses that power, but as gradual decomposition going on it may be a consequence of that change.
451. *Sulphuret of Potassium*—made by reducing sulphate of potassa by hydrogen. When solid and cold did not conduct at all; heat appeared to give a little conducting power before fluidity was conferred, but when fluidity given, then sudden and great increase of conducting power, as if it were like a metal or some other sulphurets. Needle stood stifly at the galvanometer. I think decomposition goes on at the same time, but it was difficult to tell in a close tube and where neither element would separate from fluid.
452. *Boracic acid*—fuzed on ring of plat. wire by oxy-hydrogen jet, yet did not conduct so as to affect the galvanometer in the least—two troughs used. This very remarkable; it is as bad as air if not worse. Did not become very fluid—volatilized and was carried of[f] by water, etc. of compound jet flame.
453. Flint glass by oxy. hy. flame conducted a little when very

hot and traces of decomposition; but not so much action as expected.

454. Bottle glass by oxy. hy. flame—did not seem to conduct at all.

455. These effects seem to connect very closely with possibility of decomposition shewing some relation. The glasses differ very much from Borate of lead glass, etc., for there elements for both poles.

22ND APRIL 1833.

456*. Large Electrical machine, introduced Sulphuret of silver into course of electricity so as to indicate whether silver reduced at the N. pole, i.e. that at which electricity entered; connected also an electrometer with the arrangement before the sulphuret of silver, so as to shew whether the sulphuret conducted so badly at common temperatures as to allow any accumulation which might be shewn by the electrometer. It was requisite that the electrometer be shaded from the machine, which otherwise by its great power acted by induction, etc., etc. on the glass and caused opening of the leaves a little.

457. When all well arranged, could not observe sensible opening of electrometer leaves though machine in very excellent action. I thought once I obtained little opening but it was a fallacy.

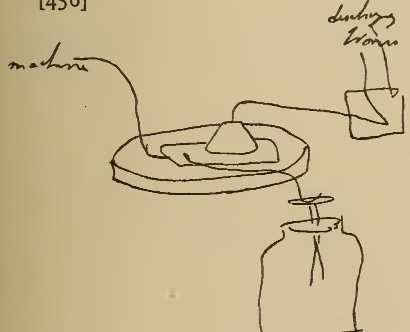
458. When machine at first weak, obtained no indication of decomposition of sulphuret by 100 turns; but on applying a little tallow and getting it up to full action and giving also 200 turns, obtained a very nice little spot of metallic silver reduced at neg. pole.

459. This expt. shew[s] the very small quantity of electricity which the machine really furnishes and shews relation of quantity and intensity also very well in some respects. It is wonderful to see the superiority of the volt. battery over the machine as to quantity.

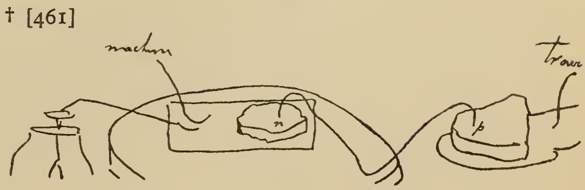
460. Fuzed hydriodate of potassa put between poles of machine—current—the Pos. pole, being the end of a wire, gave indications of brown spot, but think it was at first due to little brush or spark or effect of that kind.

461. Finally arranged it thus†, with two pieces of iodide, one connected with the machine, the other with discharging train, and the two connected by a fine bent insulated plat. wire; offering thus both P. and N. or exit and entring pole. The electrometer was connected as before. The pieces of iodide each about $\frac{1}{4}$ of inch thick.

* [456]



† [461]



462. The electrometer now opened about $\frac{2}{3}$ of an inch, the machine being in excellent action and quickly worked. If in bad action or slowly worked it did not open at all. After 100 turns there was plenty of iodine at P. Pole and none at Neg. Pole. Then changed the two pieces of iodide to be sure that nothing in them produced the effect. Still, upon 100 turns, plenty iodine at P. Pole on new piece and none at Neg. pole. This a good experiment and decisive that iodide of potassium can be decomposed polarly in the solid state like sulphuret of silver.

463. Must try dry iodide between poles of good volt. battery capable of opening the leaves of electrometer.

464. Shews relation of iodide as to conducting power also; is worse a good deal than sulphuret of silver. No wonder that when solid it does not deflect electrometer using only two or four voltaic troughs.

465. Arranged plat. poles in jackets, introduced boiled distilled water and froze it well. One vessel was $\frac{4}{8}$ of inch in width and the other $\frac{5}{8}$ of inch in width; therefore in one the intervening ice should have been $\frac{1}{4}$ of inch thick, in the other $\frac{5}{16}$ of inch, but in the first it was found afterwards near the side only $\frac{1}{8}$ of inch distant, whilst in the other it was nearly in the middle.

466. Now introduced one of these in line of discharge, i.e. the wire of platina pole was connected with the machine and the electrometer, whilst the discharging train was connected with the jacket. In this way could open the leaves of the electrometer with either in circuit; but when the machine was well and powerfully worked, the leaves with the thinner ice scarcely opened whilst with the thicker ice they could be opened nearly two inches. Hence effect of thickness and hence conducting power of ice, but small even with tension equivalent to divergence of two inches, for it was but just able to discharge the electricity of the machine when of that tension and of course was not able to discharge the whole of it at lower tension. No wonder it insulates almost the whole of that of two troughs.

467. Interesting to see what will happen on using a voltaic battery that can open leaves of electrometer.

468. When decomposing apparatus of hyd. pot. solution on paper with plat. wire was introduced into the current, then de-

composing effect took place there; at P. Pole plenty of iodine with only one turn of the machine, proving that all was right; that electricity passed; had its usual power, etc. etc. etc.

469. Four series of litmus and turmeric paper moistened in solution of sulphate of soda arranged on glass rods thus*, so that all turmeric points would receive electricity and all the litmus points transmit it. The machine gave its electricity from a needle point and the train received it at one. On working the machine it was difficult to prevent small sparks passing at the points, but at last got distinct effects, especially on the turmeric points, of alkali there; and also indeed of acid at the litmus points, but the acid effect extended rather round edge and was more dilute. Still, satisfactory and shews analogy to pieces of interposed metal, etc. etc. in volt. circuit.

APRIL 30, 1833.

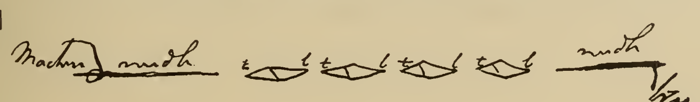
470. Have experimented to-day with a battery of 150 pairs of plates 4 inches square—double coppers—well charged—not insulated but standing on boards supported by tressels, these being damp and saline from previous acid. The action was very good—the shock strong—it would just affect gold leaf electrometer—the discharge would take place from copper to copper (wires) in the air through a space $\frac{4}{10}$ of an inch, producing a bright flame, etc. etc. etc.

471. In all the following experiments the whole of the discharge of this battery was used.

472†. The first expt. was to ascertain if ice did *entirely* insulate electricity of this intensity or if a little would pass as ought to do, reasoning upon experiments with the electrical machine and electrometer. The apparatus for interposing ice (465) $\frac{4}{8}$ of inch wide was used, though both were prepared. The plat. plate was in the middle of the ice pretty regularly.

The order was as follows: a wire from the P. Pole communicated with the wire of the platina plate in the ice—the plate touched the ice—the ice touched the tin jacket—the jacket touched a wire which communicated with a piece of tin foil, on which rested a bent wire, platina, the other end of which rested per-

* [469]



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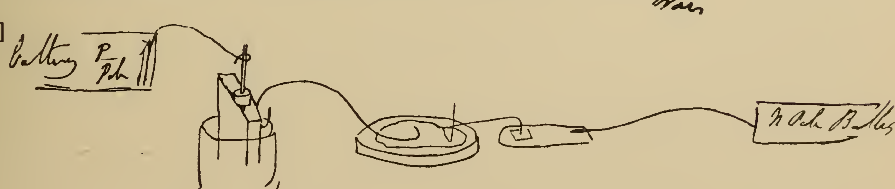


pendicularly on paper moistened with hy. potassa solution. This paper was supported on a platina spatula connected by a wire with the negative pole of the Voltaic battery.

473. At first there was appearance of chemical action and pretty quick production of a brown spot upon the moistened paper. But soon found that when connection with the ice vessel was broken there was still production of the spot and that connection with the ice did not sensibly increase it. The part therefore from the hyd. pot. paper to the ice vessel was insulated, so that no current could pass at the point of the wire through the paper and iodide but what also passed through the ice. Then found that a feeble spot was slowly formed and consequently that with a battery of this kind ice could conduct a little of the electricity.

474. It is evident the electrical machine ( ) could send much more electricity through ice than this battery, for it sent as much as the ice could carry, it being of a far higher tension, i.e. able to open the Electrometer leaves nearly half an inch, and it could produce enough to supply the possible current and still keep up the higher tension.

475. Then in place of the hydriodate of potassa the best of the two galvanometers was introduced into the circuit, the rest of the arrangement remaining the same. The galvanometer was very sensible and so nearly astatic that it vibrated to and fro in about 63 beats of my watch. In this case the current through the ice was also sensible, but the deflection was very feeble and it required to break and make contact once or twice before it was rendered quite distinct. Hence again evidence of the little ice can carry. The Electrical machine would have sent as much through by virtue of its tension, etc. and deflected the galvanometer as powerfully.

476. The moistened paper was then removed and a piece of dry iodide potassium, about $\frac{1}{6}$ of inch thick, put there instead, the positive end of plat. wire resting on it and it resting on the platina spatula. A brown spot formed slowly at the Positive pole—hence it does conduct a little of this electricity and also decompose in solid state; but electrical machine can supply as much as it can carry and form a brown spot nearly as soon.

477. As ice was interposed, that was afterwards removed and the

experiment repeated, but effect just as before—spot slowly appd. at positive point. In fact ice by so large a surface of contact make[s] up for bad conducting power and will pass as much as iodide of potassium can. When present the current passes the iodide of potassium, did not sensibly deflect the galvanometer, but probably could by reiterating the action make it sensible.

478. A piece of fuzed common salt $\frac{1}{6}$ of inch thick put in place of iodide (no ice); could obtain no trace of action by smell of chlorine or action on litmus whilst dry. Nor was galvanometer affected; current if any very small; but it is evident that the machine is a better instrument to ascertain conducting powers of these bodies by, the wires being fuzed into the salts, etc., etc. to be experimented upon.

479. Fuzed chloride of lead—put upon pos. pole—point of wire applied at the Neg. pole (no ice in circuit); could get no appearance of lead in a minute or two at N. Pole nor appearance of conduction by galvanometer.

480. Wire happened accidentally to slip off this piece on to the tin foil. Instantly violent fusion and deflagration, shewing that all the other parts of the arrangement were in order.

481. The ice being in the circuit as in the first experiment, two plates of platina were put one above the other below the tongue. On connecting one of these with the Neg. pole of the battery there was plenty of sensation, flash, etc. etc., because I was standing on the floor and the discharge took place in that direction, connecting the other platina plate with the tin jacket. I could find no additional feeling, and when I was insulated so as to avoid discharge through the feet or in other directions, then I could get no effect on the tongue, though the battery was so powerful because of the interference of the ice. Doubt whether a frog would have moved or if it did, expect only very slightly.

MAY 2, 1833.

482. As to effect of decomposing solution on polarized ray of light, can be only two directions, one across the current, the other along it.

483. Have been passing ray of polarized light through decomposing solutions to ascertain if any sensible effect on the light.

484. The light used was an argand lamp, sometimes with a ground glass globe, sometimes without. It was polarized by reflection from black glass in a horizontal plane, and the ray after passing through interposed solutions then received on a mirror reflecting it in a vertical plane and placed at the polarizing angle. Sometimes the polarized ray was examined also by a plate of tourmaline or by a prism of iceland spar instead of the second reflector.

485. The solution was contained in a square glass about 6 inches deep, 7 inches long and $1\frac{1}{4}$ inch wide. Two sets of poles were arranged and used at different times, one set placed at the different sides of the cell, the other set at the ends.

486. The first set of poles consisted of two clean plates of platina, each nearly 7 inches by 3 inches—a copper wire was soldered to each. These were put into the cell one on each side, and between them was placed a little frame of wood covered with calico, for the purpose of preventing the gas from the poles mingling with the liquid at the middle of the cell and obscuring it so that the light could not pass; but the decomposition would of course be across this partition from side to side.

487. The solution first introduce[d] was a saturated solution of sulphate of soda, and the polarized ray passed through an extent of seven inches of this solution *across* the electrical current which, when contact was made, passed from one side to the other.

488. The battery used consisted of 150 pairs of 4 inch plates, double coppers, and gave an excellent discharge spark. Yet when contact was made or broken not the slightest influence on the polarized ray could be perceived. The contact of the poles was changed from side to side but still no effects.

489. The current of electricity going across the solution was so effectual from the quantity of salt present, the large extent of discharging contact in it, etc. etc., that the spark passed very freely indeed. In fact the experimental cell was as large nearly and as effectual almost as any of the battery cells.

490. Then these poles were taken out and others of platina plate put in, one at each end. These were about 1 inch wide and 4 inches long and a slit was cut in the middle of both, to let the ray of

polarized light pass through it, now traversing the solution in the *direction of the electrical current.*

491. On making or breaking contact not the slightest effect could be perceived on the polarized ray, though the direction of the electrical current was changed and the mode of examining the ray changed also.

492. This solution was now removed, and all being well washed in distilled water, a saturated solution of nitrate of lead was used instead. But when tried both *across* and *along* as above, still no appearance of action on the polarized ray was evident.

493. I do not think therefore that decomposing solutions or substances will be found to have (as a consequence of decomposition or arrangement for the time) any effect on a polarized ray.

494. Should now try non decomposing bodies, as solid nitre, nit. silver, borax, glass, etc. whilst solid, to see if any internal state induced which by decomposition is destroyed, i.e. whether when cannot decompose any state of electrical tension is preserved. My Borate of Lead glass good, and common electricity better than voltaic—higher intensity.

6 MAY 1833.

495. *A research.* On the nature of the electric spark: the manner in which it passes is by conduction or in some new way: compare it with electricity passed through oil, water, saline solution, etc. etc. up to good conductors in quantities greater than can be conducted: does decomposition of matters in its course take place? There is no proof of lateral repulsion of its agent in its course but rather proof of attraction—see fuzed match by lightning, etc. etc. etc. etc. etc. Of course its nature in various gases, simple, compound, metallic, oil turpentine, etc. etc., heavy, light. Its comparative conduction in these different gases.

496. Have experimented further on the effect of electrified bodies on a polarized ray.

497. Put sweet oil into the large cell, and using the large poles ( ) looked at a polarized ray passed between them when one pole was connected with the electrical machine and the other with the discharging train; but could find no indication of effect

on the polarized light when the poles were electrified, i.e. put into opposite states.

498. The insulating power of the oil was not great. I could get only small sparks from the prime conductor.

499. Then arranged the poles (   ) so as to look along the direction of the electric current or attractions, but could find no effect. The distance between the poles when looking between was about 1 inch. The distance when looking from pole to pole was about 7 inches.

500. Now substituted oil of turpentine for the sweet oil. This oil of turpentine had been on the house top for a twelvemonth. It depolarized the light, as Biot has described, very well, but no additional effect was observed on the polarized ray when passed through either across or along the current. The insulation was so little by the oil of turpentine that scarcely any sparks could be drawn from the conductor.

501. Now experimented with a piece of borate of lead which I had fuzed into a glass in a platina tray on purpose. This is an excellent insulator when solid (see expts. on optical glass) and an excellent decomposing conductor when fluid. The piece was $6\frac{1}{2}$ inches long, $1\frac{1}{8}$ inch wide and from 0·2 to 0·3 of an inch thick. Conductors were applied along the two flat sides so as to make it a Leyden arrangement, and as the piece was too bubbly and too deep in colour to look through its length, the polarized ray was only passed through its width, i.e. $1\frac{1}{8}$ of inch in extent. The edges at the parts looked through were made plane by applying two little pieces of crown glass by means of evaporated oil of turpentine. No effect upon the polarized ray could be observed either when the surfaces were charged or in their natural state.

502. Then slits were made in the conductors so as to look through them *along* the line of electric attraction, but still no effects could be perceived upon the polarized ray.

503. Hence I see no reason to expect that any kind of structure or tension can be rendered evident either in decomposing or non decomposing bodies—in insulating or conducting states.

504. Does not this favour the idea that conduction does not depend upon decomposition, or else an attempt at the latter might

be expected in borate of lead; but rather that decomposition depends upon conduction.

MAY 7, 1833.

505. Acetic acid alone does not decompose or arrange or conduct, but in acetate of potassa passes to P. Pole. Hence a proof of combined action of two proximate elements present, when one only insufficient. Chlorine, iodine, sulphur, etc. etc. same thing.
506. If salt added to water, makes it better conductor and at first causes more gas to be evolved, but add more salt and as still better conductor less gas evolved; ? because difference between particle and particle not kept up, and salt gives way first as being more easily decomposed and therefore giving way at lower intensity.
507. Colour of spark at two sides indicative of decomposition—try it in other gases than air.
508. Possible decomposition of gases, etc. etc., 283. M. A. Gas. Vapour of mercury. Use spongy plat. poles and see if in M. A. chloride is formed at Pos. pole, or use poles of silver, zinc, etc. etc. etc. Silver reduced from chloride by zinc, etc. etc.
509. If decomposition necessary to conduction (in certain cases), then will not the new view explain why increasing distance of the poles retards and diminishes current and decomposition, for then general mass of attractive forces so much greater in proportion than that of the electric force super-added?
510. It surely must be true that if decomp. be prevented at *one end* of fluid conductor it will be prevented at the other.
511. Hence if one end be made so extensive that intensity too small (if there be a limit) for decomp. there, none ought to happen at the other end.
512. Then even hyd. pot. on paper, if other end carried well off and discharged by large surface of metal, should not decompose but be prevented though electricity is passing.
513. Should not many plates as diaphragms across fluid conductor prevent decomposition, yet not prevent current? Perhaps good expt.
514. De la Rive on interposed plates, Ann. de Chimie, xxviii, p. 190. Davy, Do., P. T., 1826, p. 413.

515. Expansion very much of middle of fluid conductor should also prevent by diminishing intensity there below the mark (if a limit).

516. May obtain different intensities very well by putting wires or plates into fluids carrying only a feeble current—nearer to or farther from each other—testing for current by galvanometer and trying for chem. action.

16 MAY 1833.

517. De la Rive and Davy's account of interposed plate effect probably not true; must consider them.

518. Terms electro-positive and electro-negative are terms merely relative—agrees with Berzelius and Davy's view, but the former perhaps too specific.

519. Davy says (P. T.) attraction or decomposition diminishes in force from Poles. Should not be so. Power is alike every where in equal strength of current. Grotthus says it is alike but then gives a law of attraction and repulsion; that cannot apply and would make it different. Ann. de Chimie, Old series lviii, p. 66, 67.

520. How is decomposing power modified by introducing plates as secondary poles? This important. Query the law.

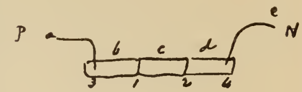
521. Is the law this (above a certain intensity, i.e. the one required for decomposition to take place at all), that whatever the size of plates or number intervening, or constant section of decomposing matter, or variable section or variable length or variable strength, or number of series in the battery; that (provided other decompositions do not mask the indicating one) equal currents of electricity measured by the galvanometer evolve equal volumes of gas or effect equal chemical action in a constant medium?

522. Is it possible it may generalise so far as to give equal chemical action estimated on the same elements on variable media? Ought it not to be so if decomposition essential to conduction?

523. De la Rive's results (Ann. de Chimie, xxviii, 190) do not at all countenance this regularity or the dependance of conduction on decomposition.

524. Acid alone ought not to travel at all. Davy witnesses that it, as every thing else, is always developed *at* pole, not in its course there.

525. If water be found to act as a pole, then will probably evolve different elements to what platina would, because in different relation to the separating bodies. Suppose *a* and *e* platina, *b* and *d* water and *c* sol. muriate soda, probably M. A. may appear at pole 1, and soda at pole 2, but then because of relation of M. A. to water *b*, decomp. and transfer take place through it and chlorine will appear at 3 and hydrogen at 4. The probability also is that no mur. acid will travel electrically to 3, nor any soda or alkali to 4. If fluids could be well confined to their places, should then have 4 places of evolution of elements: try this.



526. In a decomposing solution, the recombination of the elements transferred seems to be active there also in restoring the electrical equilibrium between any two or more particles in the line of transfer, just as Davy supposes in the case of *generating electricity*. There the disturbance by contact put in equilibrium by chem. action. Here the disturbance produced by the poles put in equilibrium by chem. action also. If conduction in decomposing cases belongs essentially to decomposition, would almost prove the truth of this view, or at least give it great support.

527. Have prepared a solution of pure soda.

528. „ „ pure Sul. Acid.

529. „ „ pure nitric acid.

These solution[s] are such that one volume of the alkaline solution will neutralize 1 volume of either of the acids.

530. Made a very weak sul. acid, barely acid to taste, put it into two glass vessels connected by asbestus. Used a battery of 4 troughs, 40 pairs of plates, charged moderately and connected by two platina plate poles as in fig. The action was very feeble owing to want of conducting power. After half an hour removed the asbestus and the poles—took equal parts of the fluid of each vessel and neutralized them by a solution of soda; they appeared to take equal quantities.

531. Now used the much stronger Sul. acid (528), operating in the same way—far more powerful action—plenty of gas at both sides—asbestus smoked. Manganese pink colour appeared directly at P. side, none at N. side. Pink tint went on increasing until end, then almost opaque; was continued for 20 minutes and then re-

moved and examined by neutralizing equal weights with the soda solution (527) measured out.

176 gr. N. fluid required 12·5 parts soda solution.
176 „ P. fluid „ 16 „ „

During the addition of alkali, manganese oxide precipitated.

532. Put on a similar expt. with other glasses—the same acid 528 and the first asbestus used, 530, which had given no pink tinge with weak acid. All worked well—plenty of action—much gas—heat on asbestus—no pink tint at the positive side during the whole expt.—was continued 35 minutes. Then fluids neutralized as before.

161 grs. N. fluid required 11·75 parts soda solution.
161 „ P. „ „ 16 „ „ „

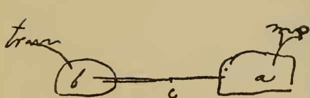
533. Put on another experiment with third set of glasses, but same acid and same asbestus as the last, and changed the plates serving as poles so that they were as in the red experiment. Plenty of action, of heat, of gas; no pink colour. Left about half an hour; then neutralized.

157 grs. N. fluid required 10·75 soda.
157 „ P. „ „ 15·5 „

534. Repeated the experiment with the former asbestus to obtain red effect but procured none; hence must have been an accident—perhaps adhering to platina foil.

535. Very evident that under these circumstances acid travels to the P. pole. The affinity of solution may do this, but must first see if any base obtained from glass or from asbestus before that can be decided. For whilst electri[fied] the Sul. acid may gather base from every part of the glass which it touches, and from asbestus also if it decomposible. As to latter, the neutralization threw down no sensible portion of earths.

21 MAY 1833.



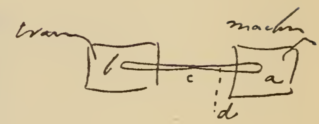
536. Experimented with Electrical Machine. On a glass plate insulated two pieces of paper; one *a*, connected with the machine, was moistened with distilled water and the other *b*, connected with discharging train, moistened with solution hydriodate potassa. Lines were drawn from each with asbestus points on the glass until the water and solution met at *c*, and then the machine was worked.

No appearance of iodine on the right or machine side of *c* could be perceived. Nor indeed ought it, for it would produce hydriodic acid.

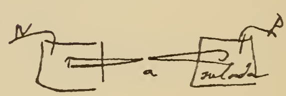
537. Made the same arrangement but used little films of asbestus instead of the lines on the glass reaching from *a* and *b* to *c*. This was to prevent flowing of fluids easily. Still no visible effect on working the machine.

538. Tested the power of the asbestos in shewing a tint by bringing a plat. wire from machine on to it; found it much worse than paper because iodine evolved entered between filaments and became so diluted in tint as to be hardly visible. Very far better on paper by same electric power.

539. Arranged the same solution and water as above, using paper points to connect by, but on working the machine obtained no signs of iodine. On applying testing plat. point from machine to parts of *a* slip between *c* and *d*, found that iodine was there present as hydriodic acid. I believe had gone into pos. paper or towards Pos. pole.



540. Now used paper as in last expt., the slips being turmeric paper, but put sol. sul. soda on P. Side and pure water N. Side. Could get no signs of alkali whilst two points touching, but on opening out the points at *a* obtained signs of alkali at end of P. slip.



541. This did not prove that before that alkali had not gone from salt at P. side towards N., and the reason why it was not visible was that enough acid, etc. was in paper or formed from the air, etc. to neutralize it, and indeed when in this expt. the end of N. side was laid on end of P. side and pressed against it, the alkali at P. end disappeared by virtue of the acid which had accumulated at end of N. at *a*.

542. Should make this expt. with pure water at N., sul. soda at P. and ends apart from the beginning.

MAY 23, 1833.

543. By putting cups and expts. in succession and sending the same electrical current through both or all, are sure that each is submitted to an equal force. Can try well this way whether the same quantity of different intensity does the same chemical work

using *same dilute S. A.* but *different sized poles*, and collecting gas, and that will tell—some poles mere wires, others large plates.

544. Also ascertain whether when different in intensity it makes difference as to substance decomposed; thus if so, then using sulphate of soda or such matters in different glass salt will give way more in one and water in another.

545. Ascertain if successive pairs of poles all acting at once will give equal vols. of gas.

546. In such vessels try different strengths of S. A. and different sized poles and see if always same volume of gas. Try also different substances, as neutral solution in one, acid in another, strong in one, weak in another.

547. Such expts. connect at once with those on intervening plates and those of De la Rive. Expect that each plate will give the same chemical effect in these latter expts., i.e. in the proposed expts. with intervening plates.

548. Expect that a plate placed any ways across a decomposing portion will give same decomposing effect if the quantity of electricity retained the same, i.e. expect that the chemical action in any possible section, plane or curved or oblique, between the poles is a constant quantity.

549. De la Rive's phenomena of polarization by plates nothing but an effect of intensity, combined with retardation of electrical current by every act of decomposition and evolution of elements. Davy wrong in his account of it. Each diaphragm plate, by evolving elements, produces a retardation, probably an aliquot part of the whole retardation due to decomposition in the pile itself. Hence the reason why diminution less by a plate with a numerous than with a scant battery. Hence why not so much in proportion by a second as by a first and still less by a third, etc. etc.

550. If quantity of electricity still retained the same, the effect would not be at all diminished, but retardation by act of decomposition lessens the quantity.

551. Important to prove retardation during decomposition because that seems at once to connect with the necessity of fluidity to conduction and decomposition, and to dependance perhaps of conduction in such cases *on* decomposition, and to what cannot be denied, the close relation of the two and their *mutual* influence.

Perhaps also it may point out that a certain intensity must be necessary and that therefore it is that thermo electricity can not decompose, etc. etc.

552. But in connection with this, is the question: is it possible that, a certain intensity being necessary to decomposition, still a lower can be conducted by the same body but now in an undecomposing state?

553. With regard to (549, 550): when the E. Machine is used where the intensity is such that the quantity is the same. Then no effect of retardation would be expected by using successive decomposing wires.

554. Extraordinary and unique relation of water to most other substances because of the electrical relations of its elements and the neutral character of the compound.

555. If no limit to decomp. by intensity and if same quantity of Electricity does same chemical work, then curious generalization as to that moving in nature—in animals, vegetables, etc. etc. etc., i.e. in bodies which can decompose (see page 1028[1] of these notes).

556. Query whether there is a *real* distinction between decomposing conducting bodies and non decomposing conducting bodies. The point will turn on a limit of necessary intensity.

557. But must still admit a very distinct character between bodies conducting and decomposing and those conducting and not decomposing.

558. On retardation of current by a decomposition, its amount, etc. etc. and its effect on repeating decomposition.

559. Equality of decomposition in every part of a humid conductor.

560. Charcoal at N. Pole refuze to combine with metals, but at P. pole unites to oxygen and perhaps chlorine and iodine. Illustrates transfer and evolution as dependant on power of union or no such power.

MAY 27TH, 1833.

561. Experimented on transfer of elements or substances alone or in combination, using sulphuric acid as the body. Employed four troughs, 40 4-inch plates Wollaston's copper, freshly charged

[1] See later, under 1 April 1838, par. 4582.

and in fair action—zinc plates had just been cleansed a little from precipitated copper.

562*. Some of the dilute S. A. (528) was put into *a* and *b*. Some of the same acid neutralized by crystd. Carb. Soda (and therefore containing nearly the same quantity of acid per bulk but in combination) was put into A and B, the quantities in the four vessels being as nearly as might be equal. Four platina plates as poles were used—that in *a* was connected with Neg. pole of the battery, that in *b* connected with that in A by a wire soldered to both, and that in B with the Pos. pole of battery. The communication with the battery was continued for 15 minutes and then the poles and asbestus (used to connect the pairs of cups) removed.

563. A solution of ammonia was now made equivalent in strength to the acid and alkaline solutions before prepared (see 527, 528, 529), and this was used to neutralize the acid solutions of this expt.

176	grs. of solution	from	*a*	required	15·1	parts amm. to neutralize them.
176	,,	,,	*b*	,,	16	,, ,,
176	,,	,,	A	,,	3	parts of the acid (528) to neutralize.
176	,,	,,	B	,,	2·8	,, amm. to neutralize it.
176	,,	acid (528)		,,	15·3	,, ,,

The two sols. from *a* and *b* after neutralization were evaporated to dryness and heated red hot, the residues dissolved in equal quantities of water and tested by Mur. baryta for S. A.; neither gave much but *a* gave most. This indicated that the electricity had found alkali even in that considered as free acid and that therefore part of the acid transferred by its means, the rest by attraction of solution influenced by electricity.

564. Arranged another experiment like the last, with four cups, using the same solutions, battery, etc. etc.; but estimated quantities better.

565. Into each vessel *a* and *b* put 17 measures of the acid free and into A and B 17 measures of the neutralized acid. Then washed the asbestus well, dried it by pressure, cut it as short as it might be to communicate well with fluids and then put it on and connected with the same battery for 45 minutes.

* [562]

566. The asbestus was then lifted out, the drops at the ends being allowed to fall each into its respective vessel, and the liquors neutralized by the sol. of soda (527). Thus two evaporating basins were balanced and then the acid from *a* put into one and that from *b* into the other. One was a little heavier than the other and therefore a small drop transferred from one to the other to make both alike. They were then neutralized.

Equal weights of *a* took 15 parts soda solution to neutralize.
" " *b* " 16·3 " "

567. With regard to A and B, the equality of weights was of no consequence because the solution was at first neutral, and all the evolved acid would be in B, all the free alkali in A.

The liquor in A took 3·2 parts of acid (528) to neutralize it.
" " B took 3·2 " soda (527) " "

568. Very evident therefore that the transfer is dependant on the mutual action of the particles.

569. Asbestus not good to connect with; it retards the current so much. Probably do far better with thick diaphragms of cloth, leather, list, felt or something of that kind.

570. Worked with the Electrical machine—making expts. of the kind (536, 539, etc.).

571. Tried to use paper moistened in oil or oil of turpentine as a pole against solutions, but they would not conduct enough.

572. On trying paper moistened in water against other paper points moistened in metallic or other solutions, found I could not trust to the solutions retaining their places, for on putting two pieces together on glass without sending current through, after 10 minutes much of the solution of lead had passed into the paper at first moistened with water only. Hence no certainty of confinement of substance and no deduction from electrical action.

573. Cannot find two immissible solutions or liquid[s] which do not act under ordinary circumstances on each other and which do conduct sufficiently well.

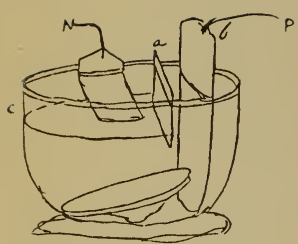
30 MAY 1833.

574. Water is a good substance as to elements for decomposition but bad as to conducting power. On putting a salt or even an acid or alkali (the latter suffering no decomposition), then con-

ducting power is increased, and the decomposition *also*. This seems to shew that the *latter* is dependant on the former and not the *former* on the latter or on capability of the latter.

575. The reason why in Davy's expts. of transfer Baryta did not pass through sulphuric acid or sulphates is that it assumes with the S. A. the solid state and is no longer in right relation for decomposition. If it could have remained fluid it would be decomposed.

576. *Water* is one of those substances which may be considered as passing either to the Positive or Negative pole according as it is in relation to acid or alkali in solution.



577. Made an apparatus for decomposition of saline solutions using water as the Neg. pole. A finger basin had a septum of mica (*a*) fixed across the upper part of it so as to make a division about $1\frac{1}{2}$ inch deep. The basin would hold ; it was inches in diameter. A large plate of platina *b* was put on one side of the septum and retained there by a glass weight or block below. Then a strong solution of Sul. Magnesia was poured into the basin behind *b* so as to make no splashing, until it reached a little above the bottom of the septum *a*. Next a slice of clean cork was cut, well moistened in distilled water and laid on the surface of the solution on the free side of the division, and a little distilled water poured carefully on to it; as soon as the cork was round about an $\frac{1}{8}$ of inch from sul. mag. solution all was left, that little adhering salt should fall to bottom of the *water*. After which more distilled water was poured on until a stratum $1\frac{1}{2}$ inch in depth *c* stood above the sul. magnesia on that side of the septum. Now a plate of platina in a horizontal position was introduced just beneath the water and connected with a cup of mercury so as to form a Neg. pole; the distance below it and the plane where the water and sul. mag. met was $\frac{7}{8}$ of an inch.

578. Connected the two poles with a battery of 40 pr. of plates moderately charged. There was action—gas was evolved—hydrogen at the horizontal Neg. pole and Oxygen at the large vertical positive one. After a little while Magnesia also appeared at the Neg. side—it was evolved at the plane where the solution and water met and not at the metallic negative pole—it could be easily seen by looking horizontally across the glass. It was

gradually bent upwards just at the middle part under the pole, being made to ascend there by the currents produced in the water by the ascension of the hydrogen gas. The currents at the P. Pole were by the mica septum *a* prevented from interfering.

579. As the action continued the layer of magnesia became more and more evident and was very beautiful.

580. Here a true water pole produced.

581. On breaking voltaic connection and taking out N. Pole of Platina, examined fluid in contact with it by turmeric paper but found no signs of alkali. On taking out the Positive Plat. pole and examining that by litmus paper for acid, found plenty there. Hence no interference of other salts or substance—the acid on the plat. and the magnesia in the water—the elements of the salt decomposed.

582. Tried the same experiment with sul. copper solution but here had different results; copper, or a black substance at least, appeared first on the Neg. platina, which gradually increased for a while and then action seemed to cease. As copper salt an acid and a per salt, it is possible that a sub salt formed which might travel properly to neg. pole in conjunction with action of water. No cloud or appearance of oxide occurred at contact of salt and water.

583. Tried also Nitrate of Lead solution. Here obtained at once a cloud formed at the metallic negative pole and diffusing in water, whilst *no* cloud formed at contact of water and lead solution. But lead can also form sub salts, and during the whole of this action *No gas* was evolved at either pole and per oxide of lead formed at the Positive pole.

584. On cleaning all things and then proceeding as far as putting the water above the solution of lead, I found I could not succeed in keeping the water quite free from lead, for portions taken up by a pipe and tested by Sul. Hy. shewed a little lead even at the surface. Hence sufficient reason for the cloud in contact with the metal.

585. Repeated the Sul. Mag. experiment; most decided and good results—as before. This salt forms no sub salt, etc. etc. and hence probably its value in the experiment.

586. The paper tray expt. form perhaps good for the lead experiment, etc. etc.

587. In Lecture to-day, on using glass cell with Sul. Soda solution coloured by cabbage, I also used two diaphragms so as to make three divisions, the poles in the two outer. The diaphragms were of several folds of filtering paper sewed together and so rendered stiff. They were inserted in their places, holding only by friction. Under these circumstances Porrett's effect occurred exceedingly well, and the fluid at the P. Pole was in less than a minute nearly $\frac{1}{4}$ of inch higher there than elsewhere. Must repeat this; I think effect will merge into a mere case of transfer and be explicable on my principles.

See on this point De la Rive, Ann. de Chimie, 1825. Vol. xxviii, p. 196.

588. Made a few expts. on substitution of bodies containing no water as exciters of the Voltaic battery. A piece of platina foil was soldered to one end of the Galvanometer wire; the other end of the Galvanometer wire was terminated by a fine copper wire. The platina was arranged so as to be nearly horizontal, then a little nitre put on to it, fuzed and the opposite copper end dipped into it. As soon as the nitre and wire were hot enough to touch mutually and also act chemically, the galvanometer was deflected, the electric current being in the same direction as if acid were present instead of nitre and corresponding to the oxidation of the most oxidable metal in the ordinary series.

589. As soon as the copper wire was much oxidized the effect almost entirely ceased—but on recleaning it was again renewed.

590. The effect was not due to thermo electricity, for on cleaning off the nitre, heating the clean platina and touching it with the copper (as might by accident have happened before), the magnetic deflection was strong but in the *contrary direction.*

591. Made the same expt. with Chlorate of potassa—there was good voltaic action at the moment of oxidation of the copper. Hence Platina, chlorate of potassa and copper form a voltaic series.

592. Made the experiment with Platina, Chlorate of Potassa and an iron wire. A current of Electricity produced at moment of oxidation. Hence this arrangement does.

593. Porrett's effect perhaps common to all insoluble substances, and perhaps fluids (different) in different cells might go different ways—try acids, alkalies—try silica—Insoluble acids—Insoluble bases as oxide of lead, etc. etc.—try metals. Many folds of bladder, the more the better.

594. Action is probably a result of general action of particles, even when attraction not enough to make actual combination or even solution. Simple wetting shews attraction and therefore perhaps that the limit.

595. Search for and apply the general principle. Is it connected with the action of superficial particles (carbonate of Soda—Iodide Mercury, etc. etc.) or of Platina when Spongy on gases?

596. Difficulty on the old theory of comprehending the decomposing action of thin plates across the solution, as gold leaf for instance: can hardly think the opposite sides are in sensibly opposite states of Electricity, so as to attract different elements by virtue of that difference.

27 AUGUST 1833.

597. Continued expts. like those of 591, 592, etc., using platina foil either with copper or with iron wire. In all these experiments the current was as in the former experiments, i.e. in the same direction as with acid—for on substituting acid the needle was deflected the same way—except with nitrate and chloride of *silver*, and these caused deflection in the opposite direction. The following are the results—

598. Platina and copper with

nitre fuzed, etc.	Voltaic action and current.
chlorate potassa	Do. „ „ „
carbonate potassa	Do. „ „ „ strong.
sulphate soda	Do. „ „ „ weak.
Nitrate silver	Do. „ „ „ *reverse direction.*
chloride of silver	Voltaic action *wrong direction.*
chlo. lead	Volt. action, strong and right.
chlo. sodium	„ „ „ „
chloride bismuth	Do. „ right, good.
chloride antimony	No deflection, though blacking of copper.
oxide bismuth	strong and right.
oxide of lead	Do. „
Iodide lead	Do. „

599. Platina and Iron with

Nitre	Voltaic action, right direction.
Sul. Soda	„ „ strong and right.
Phos. Soda	„ „ Do.
Chlorate potassa	„ „ right, good.
Nitrate of Silver	„ „ current wrong direction.
Chlo. sodium	„ „ strong and right.
Chlo. lead	„ „ Do.

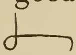
Chlo. calcium	Voltaic action,	strong and right.	
Chlo. bismuth	„	„	Do.
Chlo. silver	„	„	current reverse direction.
Iodide lead	„	„	strong and right.
Oxide bismuth	„	„	Do
Oxide lead	„	„	Do. very strong: com-

bustion of iron—fusion by reduced lead of platina.

Chlo. antimony. No current, yet iron powerfully and quickly acted upon—this body not a conductor or very bad one—that the cause probably.

600. Pursue the investigation: whether the same quantity of Electricity *always* produces an equivalent of chemical decomposition, always the same in all circumstances.

30 AUGUST 1833.

601. Made some expts. of the nature of those at (72), but instead of four feet of string used 70 feet, moistened in solution of sulphate of soda, insulated by being hung on silk supports and resting at the extremities on glass plates; and touching litmus and turmeric paper at those extremities. The Electrical machine was in good action, the former kind of decomposing wires were used,  etc. etc.; and on working the machine Alkali was freely evolved at plat. point connected with the discharging train and acid at that connected with the prime conductor of the machine. Four or five turns of the machine sufficed.

602. A piece of turmeric paper moistd., etc. was directly connected with the wire from the prime conductor. Another piece was connected with it only by means of the 70 feet of string. The point from the discharging train was made to rest sometimes on one and sometimes on the other, for five turns of the machine each time. The spot of alkali produced was so equal as to shew that, though the moist conductor was in one case one inch long and in the other 70 feet, it made no difference, since all the electricity passed in both cases.

603. Repeated this expt. with the acid evolving point; the same effects were produced.

604. Repeated it with paper moistd. in hydriodate of potassa; still the same effects.

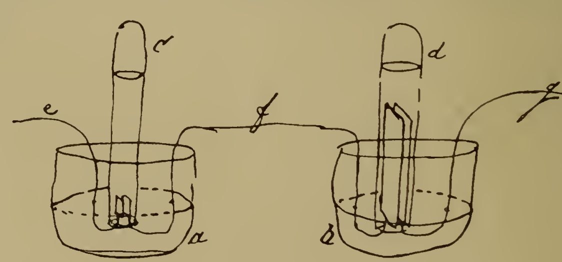
605. Now placed the machine on four glass plates so as to insulate it and made the frame altogether a negative conductor—and now repeated these and former experiments. At this negative conductor found exactly the same results, except that the points connected with the conductor, being now negative, evolved alkali and the others acid, etc. etc.; as was to be expected from the general law of dependance on the current, already recognised by myself as the only essential point.

31 AUG. 1833.

606*. Experimented on action (decomposing) of the *same* electric current on different aqueous solutions. The battery used consisted of four troughs or 40 plates 4 inches square and double coppers. It was not changed from first to last and at the end was much reduced in power. *a* and *b* were two finger basins, *c* and *d* two small gas jars, graduated. At first *a*, *b*, *c* and *d* contained a dilute Sul. acid of S.G. 1054; *e* is the wire of platina connected at pleasure with the Pos. Pole of battery and gold soldered at the end in the jar *c* to a small plate of platina 0·5 by 0·8 of an inch wide. A similar plate in the same jar was gold soldered to the end of a thick platina wire *f* which was at the other end soldered to a plate of platina 0·7 by 3·8 inches in length and breadth—a similar plate was gold-soldered to the platina wire *g* and this communicated with the neg. pole of the battery. So that there was a pair of small and a pair of large poles immersed in acid of the same strength, through each of which would proceed equal quantities of electricity. The acid was drawn off as far as could safely be done from the finger basins, to avoid decomposition as much as possible elsewhere than within and under the air jars.

607. On making battery contact, sufficient gas was evolved in less than half a minute to fill the jars nearly. On comparing their quantities there was 1·9 cubical inches in the jar *c* and 2·2 cubical inches in the jar *d*. The experiment was very beautiful and the approach to equality great. A sufficient reason for the difference was visible, for because of the smallness of the poles in the jar *c* the parts of the wires in the acid of the basin *a* gave out more gas than the similar parts of those in the basin *b*, and hence a greater part of the result of the decomposition was lost there.

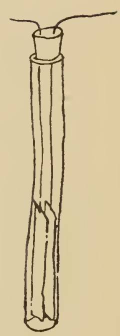
* [606]



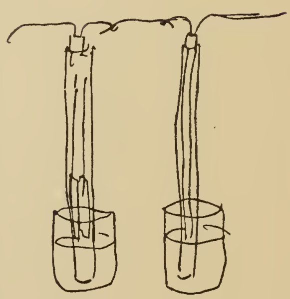
608. Hence it would appear that, whether the poles are large or small, the same current of electricity or same quantity will produce the same decomposition of water.

609. Dilute sulphuric acid was used, that the fluid might be a good conductor and yet that no other chemical decomposition than that of water should occur, least part of the decomposing effect should thereby be masked. It is true that the acid would traverse, but it was hoped the effect thus produced would be small and unimportant in the attempt thus to prove equality of decomposing actions by different sized plates.

610. To avoid source of error arising from the loss of gas evolved by the parts of the connecting wires immersed in acid not under the jars, tubes were now used, the wires entering at the *top*, so that all the gas was collected. The tubes were about 12 inches long, 0·7 inch internal diameter; the wires passed through corks at their upper extremity, the latter being rendered air tight by soft cement.



611. In one (to be called the plate tube) the poles were 9 inches long; the lower ends consisted of the platina plates 3·8 inches by 0·7, and the other 5·2 inches of each was strong platina wire about $\frac{1}{25}$ inch in diameter. In the other, to be called the wire tube, the poles were also 9 inches long but consisted only of platina wires about $\frac{1}{43}$ of inch in diameter. The plate poles therefore at the commencement of an experiment presented 9·7 times the surface of the wire poles, and as an experiment proceeded and the fluid in the tubes sank, the proportion would greatly increase until the plates also emerged from the fluid.



612. These tubes were now used with the same acid as before (S. A. of S. G. 1054), and about 2·4 cubical inches of gas collected in each by decomposing action of battery. The quantities were now very nearly equal in the two tubes; the wires had evolved rather more (about $\frac{1}{50}$) than the plates.

613. Repeated this experiment a second and third time—still the same results.

614. Now changed the places of the two tubes in the circuit; still the same results.

615. Tried again and marked the volume of gas when only half full as well as when nearly full. At the half, volume very nearly

alike; at the whole volume or larger volume, still wires had made more gas than the plates; a little only, perhaps about $\frac{1}{50}$ more.

616. The *gas* from the tubes after an experiment had a very strong electric smell, like brushes of a machine. I thought the smell was in most experiments stronger in the wire tube than the plate tube.

617. Diluted some of the acid with nine times its bulk of water, so that it contained only one tenth the acid of the other. Used the stronger acid in the wire tube and the weak in the plate tube. After decomposition the results were the same as before. Volumes of gases alike. Then used weak acid in wire tube and strong acid (s.g. 1054) in plate tube—still the volumes of gas evolved were equal.

618. The wires in all these cases evolved a little more gas than the plates, for which there may be two or three reasons. The bubbles which rise from the small surfaces of the wires are large and ascend rapidly, exposing but little surface of contact to the liquid; those from the plates are very small, rise slowly, present a large surface of contact to the liquor and are retained longer in contact. The liquor in this looks dull and turbid from small bubbles but in the other it is clear and bright. Hence there is far greater facility of solution of gas in the plate tube, and to this effect I attribute the difference of volume obtained.

619. Prepared a neutral solution of sulphate of soda; used that in the plate tube and the strong S. A., S. G. of 1054, in wire tube. The quantity of gases in the tubes *very* nearly alike. Then used the sulphate of soda in the wire tube and the acid in the plate tube; now also gases very nearly alike but wires make a little more than plates. A difference of this kind would be produced by a greater solvent power for the gases of the acid than of the neutral solution.

620. Used the strong acid s.g. 1054 in wire tube and distilled water in plate tube, taking care to wash away all former matters first. The conducting power of the water was so bad that scarcely any gas was evolved from either set of poles, but there appeared to be more gas from the wires than from the plates, and upon waiting awhile the bubbles collected seemed disproportionate. It is probable that this result is due to the extreme smallness of the bubbles from water and the solvent power of the water upon the gas. Must make a long experiment in small tubes upon the *same*

water with equal wires, etc. etc. to settle the point of equality—but is very interesting to know.

621. Strong acid S. G. 1054 in wire tube, strong sul. soda solution (12 or 14 times that before used and nearly saturated) in plate tube. Results equal as before. Then used the acid in the plate tube and sul. soda solution in wire tube—still same results. The wires their usual slight advantage.

622. The strong acid s.g. 1054 in plate tube and strong solution of Sul. magnesia in wire tube—equality of gas produced as before—reversed the solutions and tubes, i.e. putting acid in wire tube, etc. and now gases very nearly alike indeed. This supports view that difference is caused by solution of the small bubbles in plate tube, for when that tube contains substance having least solvent power, difference diminishes.

623. The magnesia was not separated in this expt. Of course the acid evolved would redissolve it.

624. There was strong electrical smell in these experiments and indeed in almost all. All I think, except those with alkali, to be described.

625. Strong sol. S. A. s.g. 1054 in wire tube—Liquor Potassæ in plate tube. Gases as nearly as possible alike—reversed the tubes as to the solutions—gases exactly alike.

626. Strong sol. of S. A. s.g. 1054 into plate tube—a moderately dilute solution of proto nitrate of mercury into wire tube. Gas as usual in the first. In second, Negative pole evolved running mercury, but *no* gas—the positive pole evolved pure oxygen, and at the end of the expt., its quantity was to the mixed oxygen and hydrogen in the first tube as 1 to 4·2. The positive wire of wire tube was somewhat acted upon and had a golden yellow appearance.

627. Put the S. A. s.g. 1054 into plate tube and a moderately strong solution of sulphate of copper into the wire tube. Gas was evolved as usual in the first. In the second, oxygen gas was evolved from the positive wire—copper appeared in arborescent forms on the negative wire, and a very few bubbles also escaped from it. At the end of the expt. the gas in the wire tube was to that in the plate tube as 1 : 2·8 nearly. This was therefore very nearly its oxygen or equal to its oxygen.

628. Strange that, with such different substances, the same quantity of water should be decomposed by the same current.

629. Would seem as if decomposing or rather separating particles might be arranged in series; thus elements which reciprocally combine, or combine with a third body, may replace and act for each other, but have no influence over those with which they cannot do so (as in Dalton's mixture of gases, etc. etc.). So decomposition of water not affected by decomposition of salts the elements of which do not combine with oxygen or hydrogen–or by acids or alkalies.

630. In that case, *Sum* of chemical decomposition *not always the same*–for with the same current passing through sol. of acid in one tube and sol. sul. soda in other, the decomp. of salt differs in amount from that of free acid, and yet decomposition of water remains the same; hence the *sum* is different.

631. But the decomposition may be always the *same* in the same solution however the size of plates, etc. etc. is changed, as is shewn in the first expts. of 31 Aug. 1833.

632. These results, if they prove correct, will shew that there is a limit of intensity beneath which decomposition does not take place, for in Solution of potassa the water gives way constantly and uniformly, but the potassa not at all. So also in sol. acid sulphuric. If in these cases the acid or alkali gave way, there should be increase of oxygen and hydrogen, by the results of potassium and oxygen added to the results of water.

633. It can hardly be supposed that such action does take place, a corresponding diminution of the oxygen and hydrogen of the water occurring, and so the sum of the whole action be equal; because that would require that the sum of *all* the chemical actions should be constant. Whereas in the case of solution of acid and Sol. Sul. Soda, it does not appear to be constant.

634. As decomposing action appears to be due to mutual action of *particles* in decomposing body, so where matter shews such action, even though it be not more than to cause moistening or wetting, still it may shew effects. This perhaps explains Porrett's expt. and others similar.

635. I think De la Rive in his paper says that with thick bladder, Porrett's expt. will not do. If my view be right it ought to do better

than thin, and will then shew that De la Rive is wrong in his theory better than any other fact perhaps.

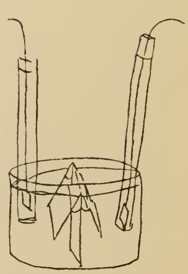
636. Several reasons against the conclusion that *conduction is due to decomposition* and in *favour* of the *reverse* seem to arise. Water for instance decomposes equally at different times, yet when pure and a bad conductor it gives way with difficulty, when saline easily. Here as conduction varied decomposition varies with it; and it does not seem right to suppose that decomposition is varied and conduction is varied as dependant upon it, because equall quantities for the electricity which passes is evolved in both cases.

637. Then make water by presence of acid or salt a better conductor and the decomposition is favoured with it. Enable it to pass more electricity and more decomposition takes place, etc. etc.

638. Subdivide electric current after passing the first or test tube and make it pass through 2 or more others; and causing these to vary, see if sum of results is then equal that in test tube. Will prove well that chemical action or decomposing power is exactly proportional to the quantity of electricity which passes in solution of the *same kind*, and so confirm result at close of third series of researches.

639. The mode of experimenting now adopted furnishes then the best method of measuring out a certain quantity of electricity when its action on other things is to be noted; i.e. decompose water by wire poles immersed in an acid solution until a certain quantity of gas is evolved; then a certain standard quantity of electricity will have passed.

640. Now wished to interpose a large decomposing plate in the middle of and course of the fluid conductor under decomposition. So as to prove that the sectional decomposition was equal to the terminal one. A cylindrical glass vessel was selected. It was divided into two cells by a plate of platina 2·8 inches by 2 inches which was cemented round the sides and bottom water tight and did not rise so high as the edge of the vessel by a quarter of an inch. A piece of mica was bent to an angle of about 90° and put over this plate of platina so that when cemented to the glass, it formed a funnel shelf over each side under which gas might be collected and retained. This vessel was then filled with a dilute Sul. acid S.G. 1031 so that no air was retained under the funnel



shaped mica. Two tubes with small poles were filled with the same acid and inserted one on each side the platina division; and then the acid was withdrawn so far that the mica ridge rose above its surface. The two portions of acid were therefore separated except by the platina and the wet mica. On connecting the tube poles with the battery of 40 four inch plates there were rendered active the two extreme poles and both sides of the intervening plate; and the gas was caught in three portions. After a time, the battery action was stopped, the gas in the extreme tubes, being oxygen and hydrogen, were put together and their volume marked; and then the gas evolved by the large intermediate plate was transferred into the same tube and its volume observed. It was much less than the first, perhaps a fifth less; it was expected to be the same.

641. On repeating the expt. the same result was obtained.

642. It was supposed this difference might be due to the smallness of the bubbles evolved by the large plate, their slower ascent and their consequent solution to a greater degree in water. Only one tube was put therefore into the vessel; its wire was made the Neg. pole and the platina plate in the lower large vessel made the pos. pole by direct contact. In this way only one decomposition took place and the oxygen was evolved at a large pole and therefore slowly. On measuring it and comparing it to the hydrogen evolved, instead of being as 1 to 2, it was about as 4 to 9, shewing the effect of solution in the water of the small bubbles.

643. This cause, and the conduction of part of the electricity by the moistened surfaces, etc. of the mica, so as never to pass through the large plate and therefore cause no decomposition, there is enough to account for the difference in the comparitive volume of gases.

644. Then changed things: put the poles 0·7 by 3·8 inches one into each tube, and covered so much of the platina plate in the cylindrical vessel with soft cement so as to leave only 0·7 by 3 inches of it uncovered, and then put in acid, etc. as in the last experiments. Now the united gas of the two tubes was equal to the gas collected on both sides of the diaphragm plate or very nearly so.

645. Drew the pole in one of the tubes near to the upper end—effects still the same. These effects are the same as those which

would be given by putting a decomposing plate of constant size across the middle of a fluid decomposing conductor and then nearer to one end.

646. Results of course always the same.

647. Reduced some Amm. Mur. plat. upon a plate of platina so as to cover it with Spongy platina, and used it as the positive pole in a test glass containing dilute sulphuric acid S. G. 1031—using also a clean gold plate as the negative pole. The power was 4 troughs; decomposition of the water was abundant. But I could perceive no signs of any transfer of the platina at the P. Pole to the N. pole.

648. Some sublimed sulphur was diffused through some of the same S. A., s.g. 1031, and used. Platina as the pos. pole and bright silver as the negative pole—but no signs of determination of sulphur to the silver. No escape of sul. hydrogen—was pure oxygen and hydrogen.

649. Diffused fine gold, which had been precipitated, through the dilute S. A. and used two poles of platina. Here after a time there were traces of gold at the N. Pole, and on changing the poles the gilded Plat. became clean; and gold, only a trace, appeared on the New N. Pole. On examining the solution by Proto Mur. tin, found traces of gold in solution—hence cause of result. Looked also for chlorine in the solution, and found trace.

650. Washing this gold well and then trying the expt. again, I could get no evidence of determination to the N. Pole. A little stuck to both but much more to the Pos. than the Neg.; scarcely any trace on latter. This due I believe to the motion of the particles and the *direction* of the striae on the surface of the poles caused by cleaning. More favourable on Pos. Pole.

651. Another reason why conduction not depend upon decomposition, given by expts. on the transference of acid alone and with alkali. In both cases, the conduction the same but the decomposition very different.

652. When discussing the action of diaphragm plates, refer to the disappearance by my theory of the difficulty as to thin plates like gold leaf, etc. (596) being highly pos. and negative on opposite sides.

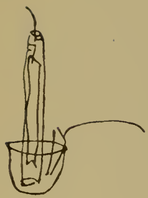
653. A middle plate connected with opposite ends of two separate

batteries, etc. etc. all easy. Merely have to consider the resultant of powers and deduce course of the final electrical current.

3 SEPTR. 1833.

654. Tried carbon in the manner found with gold and sulphur yesterday—not the slightest evidence of determination of carbon to the N. Pole. Was charcoal powdered and diffused through the dilute Sul. acid.

5TH SEPT. 1833.



655. Filled a decomposing tube with strong solution of ammonia; found that it was so bad a conductor I could obtain scarcely any gas with four battery troughs. Does not seem better than pure water. This remarkable.

656. Used a tube with two platina plate conductors very near each other so as to favour conduction and decomposition and collected the gas from *both* poles together. Still gas was evolved slowly by four troughs well charged. Apparently most at the Neg. pole, but bubbles all small. After a time, the collected gas was transferred to the water trough for examination.

657. First dry spongy platina was heated in it for the purpose of recombining all the oxygen and hydrogen evolved from water decomposed. In this way not more than $\frac{1}{7}$ or $\frac{1}{8}$ disappeared; so that very little of these gases in water proportions were present. The residual gas burnt like hydrogen. To ascertain how much was hydrogen and what the remainder might be,

4 vols. of the gas after the action of platina and

2 vols. of pure oxygen were mingled, and spongy platina heated in the mixture. The platina actually ignited and only

1·5 vols. remained when reduced to the first temperature. Hence

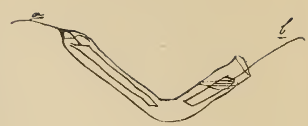
4·5 vols. had been combined to form water of which

3 vols. were hydrogen; the remaining volume derived from the original volume of gas experimented with was nitrogen mixed with $\frac{1}{2}$ a vol. of oxygen, introduced for the experiment's sake.

658. It is evident therefore that Ammonia had been decomposed, for the hydrogen and Nitrogen are evolved in right proportion, but—where do they appear?

659. Strong solution of ammonia was put into a decomposing

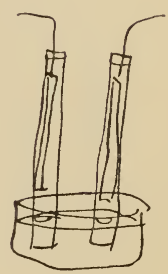
tube like that figured, about 7 inches long and $\frac{3}{4}$ of an inch in diameter. Some pulverized Sulphate of ammonia was added and shaken with it, in hopes of giving conducting power without interfering with the decomp. of the ammonia, and then the wires *a* and *b* connected with the battery, so as sometimes to collect the gas from the P. pole and at others that from the Neg. pole. Decomposition now took place *freely*; the Neg. pole gave most gas; it was principally if not entirely hydrogen? That from the pos. pole being examined proved to be almost entirely nitrogen, for on adding oxygen and heating spongy platina in it, it scarcely suffered any diminution. Is a very good expt. Shews place of Nitrogen at Pos. pole.



660. Make a good class expt. probably.

661. Strong *Nit. Amm.* solution in a tube containing both the poles decomposed readily. The gas, being treated with Spongy platina, diminished to about one half—the residue contained no nitrous gas or fumes—did not burn—supported combustion better than air—seems to be mixture of oxygen and nitrogen. Look therefore for pole at which Nitrogen evolved.

662. Strong Sol. Nit. Ammon. decomposed in separate vessels, gas collected at each pole. Much more gas in P. vessel than in N. vessel, but bubbles small in N. vessel and therefore absorption there to greater extent than in the other.



663. Finally gas in Pos. vessel equalled 12·5 vols. and was Oxygen
gas in N. vessell „ 3·5 vols.

The latter gas was examined
3·7 vols. of it and
1·9 vols. of oxygen were mixed, making
5·6 vols. which by platina and heat diminished to
0·65 vols. so that the diminution was
3)4·95 vols. of which $\frac{2}{3}$
1·65
2
is 3·30 vols. being the hydrogen present; the difference
0·4 between this and the first vol. of 3·7 is therefore nitrogen.

664. As the gases of the two tubes of this experiment, if mingled together, would not make such a gas as that obtained in the first

expt. ( ) on Nit. Ammonia, it would seem to shew that circumstances vary the results obtainable from this salt. Probably a nitrate has been formed; however, it shews very well that Nitrogen did go to the Neg. Pole.

665. It is perhaps possible that ammonia has been formed, for there is not enough hydrogen at the N. Pole I think to tally with the portion of oxygen that may have been expected from water decomposed.

666. Try fuzed Nit. ammonia some other time.

667. Strong colourless Nitric acid was submitted to decomposition in a tube like that last described. There was free conduction and decomposition. Much gas at the Pos. pole; it was pure oxygen. There was *no* gas at the Neg. pole, no solution of the platina, but nitrous acid was formed there. The acid became yellow, orange, etc. and red fumes arose from it.

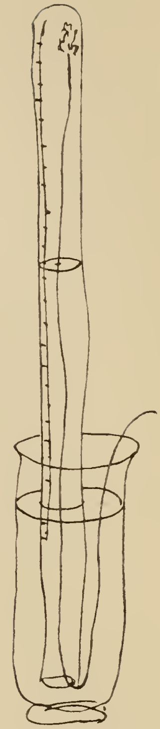
668. Nitric acid a case therefore w[h]ere a simple principle is evolved at one pole and a compound principle at the other. Proto chloride of tin is a similar case; but there the compound principle results from combination, here from decomposition.

669. May be possible to obtain some new compounds on this principle.

670. Very likely to obtain new compounds by selecting poles that are to combine or be combined.

671. Experimented with a charcoal pos. pole and dilute Sulphuric acid to ascertain and prove if the carbon will so combine with the oxygen. Plenty of gas evolved. This agitated with pure potassa, about $\frac{1}{3}$ was absorbed; it was *carbonic acid*. The rest, when a light was applied to it, burnt with a feeble explosion and blue flame; it was almost entirely *carbonic oxide*—a little oxygen was with it. Good.

672. Least I should hereafter forget, the following is the mode in which I operated with the spongy platina over water—IT IS GOOD. A piece of plat. wire had the end bent two or three times—stuffed full of Amm. Mur. Platina and decomposed in spirit lamp. The tube selected to contain the gas, of which the oxy. and hydrogen were to be combined, was rather thin, about 6 inches long and $\frac{1}{3}$ of inch in diameter. The gas being in it, the knob of Spongy platina was passed through the water to the upper end of the tube,



the lower end of the plat. wire bent round the edge of the tube, and a test glass then used to transfer the whole from the trough. The upper end of the tube was wiped and warmed, or heated even, with a spirit lamp, the water driven off from the spongy platina, the expansion of the volume of gas noticed, and when contraction began the lamp removed—the combination then went on till complete. Then the whole was cooled; and taken to the water trough and the test glass with the wire and Platina depressed, removed and set aside until wanted for the next expt.

673. When any gas had to be tried for hydrogen, Oxygen was purposely added and the expt. made as just described.

674. Gas was tried for oxygen by hydrogen in the same way.

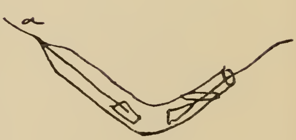
675. All the precautions requd. are not to fill the tube more than one half with gas, that room for expansion may be allowed—not to heat more of the tube than is absolutely necessary, for that only makes a great bulk of steam—not to heat suddenly, but gradually, and to cease heating so soon as the gas begins to contract, least in mixtures containing much oxy. and hydrogen explosion should be caused. But the Platina will often become red hot with exploding the gas, the steam formed preventing the latter effect.

676. Private references for Paper on Electro chem. decompos.

At Par. 538 refer to P.T. 1807, p. 22, 24 exp., pp. 25, 30, 41.

„ 551 „ „ 1807, p. 41.

6 SEPT. 1833.



677. Thinking that a dilute N. A. would perhaps give nitrogen at the N. Pole, some strong acid was put into a tube and then gradually diluted, being tried each time by connection with a nearly exhausted voltaic battery of four troughs, *a* being made Neg. pole. When strong, oxygen was given at Pos. pole and no gas but nitrous acid at Neg. pole. When weak (about half water), the decomp. went on as in water, oxy. at Pos. pole and hydrogen at Neg. pole, with no nitrogen apparently.

678. As dilution proceeded it was found that each time the battery contact was made with the wires, a little gas proceeded at first from N. Pole and then ceased, though contact was continued. As the water increased in proportion this first evolution at N. Pole

increased also, but still ceased after a second. Without adding water, if the contact were broken for a while and then renewed, the same effect took place. After a while, it was traced to the greater power of the battery from accumulation, at the moment of contact being able to effect some decomposition which could not be done by the constant power of the same battery (for it continued in the acid the whole time); and there is no doubt a stronger battery would have continued the action, evolving gas at the N. pole.

679. It is to be remembered also that the weaker the acid the greater the effect; i.e. weakening the acid would produce the same effect as strengthening the battery.

680. Then collected by repeated contacts enough of this gas to examine, and found it was pure hydrogen—resulting from the decomposition of water. Hence explanation of the whole; when enough diluted the weakest state of battery decomposed the water in preference to the acid. When strongest the acid decomposed first and easily with the weak battery, and to decompose the water in a strong acid required a stronger power than for the acid itself; but the difference diminished by diluting the acid and at last the water gave way in preference.

681. Hence by adjusting strength of acid and of battery, either the water alone, or the acid alone, or both together, can be decomposed at pleasure.

682. Now to get Nitrogen at N. Pole tried fuzed Nit. ammon. in a tube; evolved gas freely. The gas at N. Pole burnt like hydrogen, but 5 vols. with oxygen and platina indicated 3·4 of hydrogen. The 1·6 I believe was nitrogen—but I am not quite sure it was not nitrous oxide. Think that here Nitrogen went to N. Pole.

683. Now decomposed Nitric acid strong in which Nit. Ammonia had been dissolved, adding also a little water. A little gas was evolved at N. Pole and I continued the action until enough was collected to examine; but on removing apparatus from the battery, still evolution of gas went on from both P. and N. Pole; they appeared to act as nuclei, evolving a gas from the liquid with which it had become charged by the action of the battery. This gas by prot. sul. Iron was not nitrous gas, nor was it hydrogen;

it extinguished a flame and was I believe pure nitrogen, but I am not sure it was due to the electro chemical action of the N. Pole, otherwise than as that solid substance caused its disentanglement from the liquid, etc. Hence proofs of determination of Nitrogen to the Neg. pole not good or clear.

7TH SEPT. 1833.

684. What is the dependance of decomposition upon intensity and conduction? An intensity not being sufficient when conduction bad, will it become sufficient when the latter rendered greater. If so, is a fact against theories of decomposition of fluids, etc. by attraction of poles.

685. Motions of mercury, etc. etc. of Davy, Herschell, etc. perhaps an intermediate state between Electrochemical transfer of the ordinary kind, and those cases classing with Porrett's effect—examine this.

686. *Unipolarity references*—
Erman, Ann. de Chimie, lxi, p. 113.—Ann. de Chimie, lxi, 116, 122; 1824, xxv, p. 278.
Davy, Elements, 168.
Brande, P.T., 1814, p. 60.
Biot. Ency. Brit. Supplement. Vol. iv, p. 444. Column 1, Article Galvanism, A.D. 1824.

687. *Mercury motion references*—
Davy, Elements; P.T., 1826, p. 417, expt. to make.
Herschell on Do.—his reasoning, Phil. Tr., 1824, p. 162, 153.
,, his distinction, p. 168 and note, p. 169; very suspicious.
,, mechanical action proportionate to absolute quantity passing, p. 168 and note.
,, Direction of currents, 179.

688. Harris on screens in Magneto electricity, Phil. Trans., 1831, p. 72, 497.

689. Interposed plates in humid conductors.
De la Rive, Ann. de Chimie, xxviii, 190.
Davy—Phil. Trans., 1826, p. 413.

690. Insulating power of water.
Harris, p. 6 of his paper.
De la Rive on Do. and chlorine, etc. etc.
Davy, Phil. Trans., 1807, p. 45.

691. Conducting power of metals even alter by a little alloy added to them. Harris, Phil. Trans., 1827, p. 22.

692. Theories of cause and source of Electricity: references, etc.
Brande, in contact of Metals, Elements Chem. pp. 211, 263.
Wollaston, in chem. action, Phil. Trans., 1800.
Davy, his views, Elements Chem. Philosophy, 163.
" " Brande's Elements, p. 266.
" Metallic contact not necessary, P. Tr., 1825, p. 337.
" Other effects, P.T., 1825, p. 339.
Biot, in contact of metals, Ency. Brit. Supp. Article Galvanism.
Becquerel and Ampere, in contact. Ann. de Chimie, 1824, p. 29, xxvii.
Moving electricity—matter or vibration? Savary, Ann. de Chimie, xxxiv, 54.

Ritchie quotes two or three expts. against chem. Theory which are worth repeating, P.T., 1829, p. 364, Exp. iii, iv, v, and writes on the cause, but very loosely. Another paper of his, P.T., 1832, p. 279.

Pfaff.
Biot, Absorption of oxygen by a pile in air.
Nobili, on production of Electric currents and their nature, Bib. Univ., xxxvii, pp. 11, 24, 118.
Davy, on relation of Electrical and chem. changes, P.T., 1826, p. 383.

693. On Electric light, Biot, Ann. de Chimie, liii, p. 321, 1805.

694. There is no reason to doubt the compound nature of the metals *because* of their habits in the Voltaic circuit; for several sulphurets known to be compounds have the same habits.

695. All bodies under the law described in fourth series are compound bodies and decomposable. This remarkable; is it very strict?

696. Reasons why conduction does not depend upon decomposition.

697. Because conduction often exists without decomposition, and that not in bodies supposed to be simple only, but also in those known to be compounds, as some metallic sulphurets—chloride of antimony, etc. etc. Metals as simple bodies—but decomposition never occurs without conduction.

698. Because in bodies that do decompose when they are conducting electricity, the latter is not in proportion to the former—for when conduction or the quantity conducted is the same, the quantity decomposed varies.

699. Because that certain bodies would seem to be of a nature (by their elements and proportions) to decompose if they could only conduct, but they do not conduct. Boracic acid—Acetic acid—Ammonia strong sol., Water, etc. etc.

700. Because substance added to increase conducting power is not itself decomposed—both compound and simple bodies will do this—salts, chlorides, chlorine, Iodine, bromine, etc. etc.

701. Query the action of the following bodies in assisting the decomposing power of water. Will substances not decomposing alone, as sulphurous acid, etc., produce increased effect on water? S. A. is of this kind. Will sugar, gum, starch, etc. do? Sul. Hy. contains elements both of which go to one pole; will it do then? Will Prussic acid do?—it makes a solution lighter than water. Has the little S. G. of ammonia sol. anything to do with its non conducting power? Will Boracic acid dissolved do? If so, it will shew that effect is not due to liquefaction by solution, as liquefaction by fusion does nothing with it. Strong phosphoric acid. Strong Sul. acid.

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702. Made an expt. on magnetization of Iron, relative to the following consideration. If when Iron becomes magnetic electric currents are caused round its particles, all parallel to each other, or if electric currents before in all directions are then put parallel, then there should be contraction of the iron in the direction of the magnetic axis in consequence of the mutual attraction of these currents; and perhaps also expansion in directions perpendicular

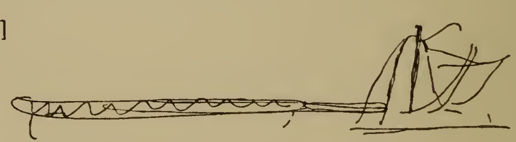
to this. A bar rendered magnetic should become shorter and thicker it may be, or undergo some change in figure and perhaps volume; and though attraction of spirals of a helix are not very strong, yet in a magnet the currents are very powerful, very numerous and *very close together*. Hence expect a sensible sum of action.

703*. A soft iron bar, cylindrical, 0·9 of inch in diameter and 4 feet long was covered with calico and then with one continuous helix of copper wire $\frac{1}{20}$ of an inch in thickness. There were 684 coils in the helix; each was 3 inches in circumference and consequently 2052 inches or 171 feet of wire were used. This was adjusted endways against a Smeaton's pyrometer in which the bearings and fulcra of the lever and index were as 10 inches to $\frac{1}{4}$ of an inch and 10 inches to half an inch, so that the motion was magnified 800. All was so blocked up that no motion of the index could take place except from expansion or contraction of the bar; and the bearings were such as to shew either expansion or contraction, the index being in the middle of the scale.

704. A voltaic battery of 4 trough, 40 plates 4 inches square, in good action, was used, the discharge being made through the whole length of the coil. At first no effect either on making or breaking contact could be perceived.

705. On moving the index one way or the other by the finger, it did not return to the same middle place, but always stopped a little short of it on the side towards which it had been forcibly carried. Now when placed purposely in one of these sensible situations (from which a little vibration on the iron always made it start towards the middle point), the moment battery contact was made the index moved towards the middle point. Or if contact were continued and the index replaced in either of these situations, then *breaking contact* caused the needle to move in the same direction as making contact would have done. *But no touching with one wire of the battery had any such effect.* Hence it is evident that making or breaking contact affected the iron at the moment so as to make the action sensible on the mechanical measurer; but it is also evident that the action was momentary, for the current being continued, still the index could be restored to the same place as if no current were there.

* [703]



706. It seems to me as if the whole depended upon the capability metals possess of expanding or contracting in bulk, and remaining expanded or contracted when the power was removed. Mr Fisher observed in the northern regions that if Iron bars, much cooled, were raised a little in temperature they did not expand fully, but a few taps with a key made them settle into their right bulk; and the same occurred in contracting, a sluggishness being about them. Now the index of the pyrometer I used is retained up against the bar, lever, etc. etc. by a spring—in moving it one way the bar is more compressed, in moving it the other it is less compressed than when the spring has it[s] fair action; and the non return of the index to its medium place seems due to the bar finding a difficulty in acquiring its true bulk under the pressure, finally giving.

707. When the bar is rendered magnetic, the shock, jar, concussion or what not on the particles seems to due [? do] this. Or if, being magnetic, it is compressed or expanded, then the shake or jar of particles when the[y] return upon breaking the current to their first position also seems to have the same effect. It seemed evident that, either magnetising or unmagnetising, it produced the effect; but making the particles vibrate as in producing a sound would have done the same thing.

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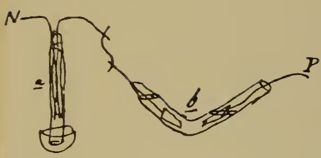
708. Have been comparing decomposition of M. Acid and water together as to the equivalents of elements evolved by a given current of electricity. Used one tube *a*, that of (611), for decomposition of water acidulated by Sulphuric acid s.g. 1054; both poles being in one tube but kept apart by a piece of glass put across between them. For the Muriatic acid, which was the purest strongest solution, used the tube *b* (659).



709. After decomposition by four troughs had gone on until there was enough gas in *b* to reach nearly to the an[g]le, the action was stopped and the gases examined. That in *a* was mixed oxygen and hydrogen; it equalled 48 volumes. That in the close end of *b* was hydrogen and = 33·2 vols., being rather more than $\frac{2}{3}$ of the first or the hydrogen present from water. Plenty of chlorine was evolved at the P. pole of *b* and there was also action on the platina pole.

710. Repeated this experiment.

Tube *a* gave 51·5 vols. of oxy. and hy.
Tube *b* „ 35·25 vols. hydrogen,

being again rather more than $\frac{2}{3}$ of the gas in *a*, i.e. the hydrogen from water.

711. In *a* the *p.* and *n.* poles together expose about twelve times as much surface as the N. pole in *b*. Hence bubbles in the first small, in the second larger—hence more solution of gas in the first—hence a cause of the slight difference observed.

712. Now sent the electric current in the reverse direction so as to collect the gas from Mur. acid at Positive pole. Very little was there evolved.

In the water tube *a* there were 65 vols. of Ox. and Hydrogen.

In M. A. tube *b*, at P. Pole only 0·35 of vols.; and that did not appear to be oxygen. Hence all the gas at P. Pole was chlorine; part was dissolved, part evolved as gas was dissolved by agitation in water and part acted on the platina. The M. A. left bleached powerfully. No water had been decomposed here, yet the hydrogen evolved equivalent to that of water—very curious and beautiful.

713. Repeated the experiment.

The tube *a* gave 72 vols. O. and H.

The tube *b* P. Pole only 0·57 of vol. of a gas left unabsorbed, and it had properties of common air or bad air. Evolved probably by solution of chlorine in the M. A. and in the water.

714. I remarked in the last expt. that whilst *b* tube was examined, the bulk of gas in *a had diminished.* Therefore put *a* on alone, and by battery evolved gas until it was full. Being left to itself, this gas gradually went on diminishing and after 3 or 4 hours not a fourth part was left. At first (12 o'clk.) there were 116 parts and at last, 5 o'clk., there were only 13·5 parts. Think this must have been an effect of permeability through the cork at top, by wires, etc. etc., but must examine it closely and also used [? use] tube hermetically sealed at the top.

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715. To-day examined the 13·5 parts left yesterday. By heating spongy platina in it the gas diminished to 2 parts, and these appeared to be like air, but was so small it was difficult to examine.

716. Hence think it cannot be due to permeability of cork, etc. etc., for no sensible portion of air has entered. Think it must be due to recombination of the oxygen and hydrogen in some way.

717. Put some pure hydrogen into the tube which had been used in these experiments, but there was no indication of permeability of the cork. No diminution of the volume of pure hydrogen. Another reason why disappearance above due to combination.

718. I then put mixed oxygen and hydrogen into the tube over water, but there was no appearance of diminution in 10 or 15 minutes.

719. Now refilled the tube with the dilute sulphuric acid S. G. 1054 and by the battery decomposed enough to fill the tube nearly. On disconnecting the battery, the effect of diminution instantly became sensible. Put the whole under water, both tube, acid, vessel, etc. etc., and left it to see if still go on—for here no escape by permeation through cork or by wires could occur. This was about ½ p. 4 o'clk. P.M. The effect still proceeded. This was in tube *a*, 611.

720. Now prepared another tube *c* similar to tube *a*, i.e. having

two platina poles or plates fixed in it, but these were hermetically sealed in through the glass, so that no cork was here. Tried the tightness of the wire passages or places; all seemed right. The plat. poles were about 3 inches long as plates and above that part were continued by wires (soldered on with gold) for about 3 or 4 inches more.

721. This tube was filled with the dilute S. A., s.g. 1054, and then connected with the battery until filled with the gas evolved. On being separated it was found that diminution of the gas went on just as fast as in the tube *a* having a cork termination.

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722. Last night at 9 o'clk. P.M. I marked the place where the gas stood in the tube *a* under water. At 10 o'clk. A.M. to-day, on examining, it had risen about the third of an inch; the effect is still going on slowly.

723. Last night at 9 o'clk. P.M. I marked the place of the gas in tube *c*. At 10 A.M. to-day, found it had risen about $\frac{2}{3}$ of an inch, and on watching it for half an hour, found the effect was still proceeding slowly.

724. In each of these tubes more than $\frac{3}{4}$ of the first quantity of gas has disappeared.

725. I suspect the cause of all this is some combining power possessed by the platina of the poles—perhaps given to it during the decomposition—for De la Rive has remarked case of peculiar effects conferred on the poles in such cases. This will account for slowness at last when the gas has risen so high that the plates are covered with water, and only wires, and that in small portions, exposed to the gas.

726. Must ascertain whether both poles or only the positive has this power; and whether it has power over other gas than that evolved by the electricity. Also whether washing the poles in water (as in transferring) removes this power.

727. If poles have this power the effect will immediately connect with that of spongy platina, and probably explain it. Perhaps merely digesting platina in dilute S. A. or at least in N. M. A. may give it this power. The poles used in constructing tube *c* had been used in decompositions, but before they were fixed in tube *c* they

had become waxy and were made red hot in spirit lamp flame. After this they were used to decompose water only once, yet effect went on rapidly in the tube.

728. Probably heating in air—or in flame with little mur. ammonia vapour—or in chlorine, etc. etc. will give this power to platina in plate or lump. Probably also heat much assist it. Try all this.

729. It is evident from expts. on strong solutions that both Ammonia and muriatic acid decompose by battery *more freely* than water. The same is the case with Nitric acid.

730. It is probable that a solution of M. A. may be obtained which, like nitric acid, will by strong battery action have part of its water decomposed at the same time with the Mur. acid, and by a weak acid [? battery] will have the latter only decomposed. If so, see whether that solution will by strong and weak battery give always a constant sum of decomposition, i.e. if when water and acid give way together, only as much hydrogen is produced as when acid only is decomposed. Should be so, but then how stands the case of dilute Sul. acid and solutions of sul. soda, etc. etc.?

731. Connect the decomposition of *water* by the hydrogen with decomposition of *M. A.—M. A.* by chlorine with *chlorides of lead, tin*, etc. etc. *Metallic chlorides* by their metals with *salts* of those metals, as acetates, sulphates, etc. etc. Acetate of lead a good salt for proportion of lead.

732. Will not white hot diamond conduct? If so, may perhaps crystallize carbon at white heat by power of the voltaic battery.

733. Continued the expts. on the recondensation of the evolved gases. Put dilute Sul. acid into a small earthen ware pneumatic trough. Prepared the tube *c* by filling it with the acid s.g. 1054, and then, connecting it with the battery, decomposed enough water to fill the tube with gas nearly. Transferred this gas into a plain glass tube without any plat. pole in it and transferred a mixture of O. and H. from Chlorate of potassa and Zinc with S. A. into tube *c*. The gas in the plain tube underwent no change, but that in tube *c* began to diminish; and in half an hour not more than $\frac{1}{3}$ of whole quantity was left, and the acid had risen above the platina poles.

734. Hence evident that the recondensation is not peculiar to the gas evolved by the battery, or due to any action of its constituents

exerted upon each other; but that it is due to an action exerted by one or both poles and can be effected on gas obtained and mixed in the ordinary way.

735. The mixed O. and H. in the plain tube, which had undergone no change, was divided into two portions and put into two plain tubes, all these operations being over the acid pneumatic trough. Then two plates of platina connected with the battery were immersed for a while in the general mass of acid, so as to decompose the water, and after a minute or two were separated from the battery and put, one into one of the tubes and the other into the other, so that the metal should be quite up in contact with the mixture of gases. This was done at 5′ past 12 o'clk., and at that time there were 9·5 vols. in the P. Pole tube and 10·75 volumes in the N. Pole tube.

736. Diminution instantly took place in the P. Pole tube, but none at all in the N. Pole tube. In 10′ the gas in P. pole tube had diminished to 3 vols. and in 25′ to 1 vol., very little of the platina being then uncovered by the acid; but in the N. Pole tube no change took place.

737. Now took the N. pole out of its tube and made it a positive pole in the general mass of acid for about 2 minutes, returned it to its own tube and observed the effect. Immediately condensation of the gas took place: in 3′ the 10·75 vols. were reduced to 6·5 vols—in 8′ to 3 vols., and in 17 minutes to less than 1 vol., and then very little platina remained uncovered by the gas [? acid].

738. Now put some more Oxy. and Hydrogen into the first Pos. pole tube so as to make 11·5 volumes. Condensation occurred, but only slowly—very slowly. Hence it appeared that the pole had lost the power it first possessed, either spontaneously by time, or by action of the acid, or by exerting its power on the gas; but it is quite clear that the P. pole has peculiar power of causing oxygen and hydrogen to combine.

739. Now tried the effect of the two poles, after being separated from the battery and washed, upon a fresh portion of acid, connecting them by a galvanometer so as to see if any current were produced. The deflection was such as to shew that the Pos. pole was to the neg. pole as platina is to zinc, i.e. as the less oxidizable metal—and either plate of platina could have these properties conferred upon it by being made the pos. pole for a while. It

required, however, contact for some time, for an instant contact did not do so much as contact with battery for a minute.

740. Used spongy platina in acid against ordinary platina but could not find that it caused deflection of the needle. It could be made either pos. or neg. pole and then acted like a plate of platina made pos. or negative.

741. These experiments shew cause for the deficient quantity of gas in many of the former experiments.

742. In constructing a volta electrometer, may obviate the error this effect produces by making the wires long, or making them enter below and not evolving so much gas as shall reach to them; consequently no secondary action will then go on.

743. In investigating the effect by other metals, as gold, silver, platina, palladium, Rhodium, Mercury, try them also in solutions of alkalies, Neutral salts and in N. acid and M. A. and Acetic acid, etc. etc. Try also action of the P. Pole so prepared on other substances than Ox. and Hy., as *Hy. and Chlorine*, *Carb. oxide and O.*, *Carb. oxide and Chlorine*. Perhaps may obtain new compounds. Try also platina prepared by sol. in N. M. Acid, heat in gases of different kinds, etc. etc.

744. Will a pole which has not decomposed but which is in connection with the P. end of battery do any thing? Is not likely, but try.

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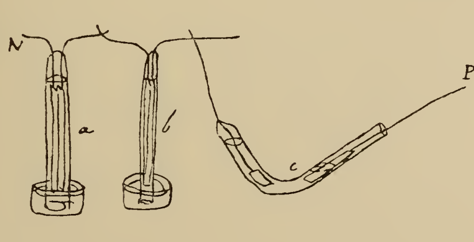
745. As to peculiar effects of the P. and N. Pole, will Pos. pole act as less oxidable metal to a piece of platina in common state? If so will N. Pole act as more oxidable metal to Do.? If so then both poles in peculiar state, but opposite to each other, and may consider that whilst P. Pole has power of combination when alone, N. Pole has probably power of decomposition or something equivalent to it. Extend this view by thought and experiment.

746. No doubt that different sides of same plate or even different parts of same side may have these powers induced upon them.

747*. Arranged three tubes: two of them, *a*, *b*, had each two plate platina poles within, but *c* had only one. The wires were in all cases fuzed in the glass, corks being no longer used for that purpose.

* [747]



748. A strong solution of sul. soda was put into *a*, Dilute Sul. Acid s.g. 1054 into *b*, and strong solution of muriatic acid into *c*. N. was then connected with the N. Pole of a battery of 4 troughs, but nearly exhausted and acting very slowly, and P. was connected with Pos. end. The action was continued until more hydrogen could not with safety be evolved in *c*, and the battery then unconnected. The bulks of gases were then noted but the tubes left. Diminution instantly began in *b* or that with Sul. acid. It did not happen in *c* where there was only a neg. pole and hydrogen, but it did slowly occur in *a* or sul. soda tube.

The O. and H.	from *a* or sul. soda	= 75·5	volumes
The O. and H.	„ *b* or dil. sul. acid	= 72·5	„
The H.	from *c* or M. A.	= 50·0	„

The hydrogen from the M. A. is therefore the same in quantity as that from Sul. soda solution, and the deficiency in the S. A. tube has been caused by the combination of a part of the O. and H. by the P. pole even whilst decomposition going on.

749. Still, it is evident that an equal current of electricity evolves equal quantities of hydrogen in these three cases.

750. Also evident that effect of P. Pole occurs in Sul. soda, but very slowly as compared to that in acid.

751. Made the expt. again in the same order and with the same solutions, but now used a newly charged battery acting well and powerfully, and consequently allowing only a short time for the P. pole to act on gas in S. A. tube before bulks were estimated. Now

The O. and H.	from Sul. soda	= 74·8	vols.
The O. and H.	from Sul. acid	= 72·9	„
The H.	from M. A.	= 49·75	„

Here again the H. evolved in the three cases evidently the same in quantity, and now the S. acid tube differed less from the other than in the former case.

752. Must make an S. A. tube in which gas when evolved is no longer in contact with Poles.

753. Now sought for an M. A. in which water should also give way as well as M. A. 2 vols. strong solution of M. A. and 1 vol. distilled water—using four batteries, gave nearly all chlorine at P. pole; think that the little bubble left at last had been air evolved from the water.

1 vol. M. A. + 2 water—chlorine only at P. Pole.

1 vol. M. A. + 4 water— Do.

1 vol. M. A. + 9 water—a little oxygen appeared with the chlorine at P. Pole.

1 vol. M. A. + 15 water—a little oxygen, much chlorine at P. Pole.

1 vol. M. A. + 15 water, with a little strong sul. acid added to increase conducting power and action on the water, for I think that the M. A. and water alone in these proportions conducted badly. It now conducted better, but there was not more oxygen in proportion evolved; the little produced was good oxygen. Plenty of chlorine was evolved.

1 M. A. + 24 of the usual dilute S. A. (S. G.)—much chlorine, little oxygen.

754*. Now put the dilute S. A. s.g. 1054 into tube *a* and the mixture of 24 of this sul. acid with 1 Mur. acid into tube *b*, to see whether, w[h]ere solutions gave such different results at P. pole, they would be the same at N. pole. Connected both in the order they are placed in with the N. and P. poles of battery, and when enough gas evolved examined them.

a or the dilute S. A. s.g. 1054 gave . . . . . 34 vols. hy.
b ,, ,, with M. A. in it gave 33·8 vols.

These numbers prove that the quantities are really the same and, as I thought, the difference was due to the insulation of *b* not being so good as *a*; and having been touched by the wet hand, I changed the order of the solutions, the tubes remaining in the same places, and put the mere dilute S. A. s.g. 1054 into *b* and the muriated S. A. into *a*. After decomposition

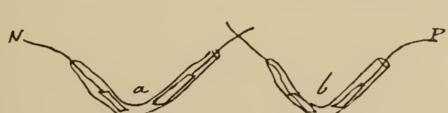
a or Mur. sul acid gave 34·5 vols.
b or simple sul. acid ,, 34·3 vols.

The difference is the same as before for the tube, but reversed for the solutions, and shews therefore cause, and also exact equality of hydrogen evolved.

755. Must be careful in the Volta electrometer to insulate it and the connected apparatus well.

756. Now tried to obtain a Mur. acid which, like N. A. diluted, should by weak battery have M. A. only decomposed and by strong battery have part of the water go with it; so as to illustrate

* [754]



intensity and also shew whether, when both decompose, there is only the same quantity of hydrogen evolved.

757. 1 M. A. strong solution + 100 dil. S. A. s.g. 1054—gave fair proportion of oxygen at P. pole and plenty of chlorine, using a strong battery of 40 four inch plates.

758. The same mixture, using only 5 plates of the battery, gave also oxygen. Cannot say whether it gave less in proportion than before; think not. It ought however if water had ceased to decompose by weaker battery.

759. On using on[ly] 3 pair of plates, decomposition seemed to cease altogether. This refers to intensity and probably make a good experiment. Query: did the current cease at *same time* or would a consecutive M. A. solution have decomposed?

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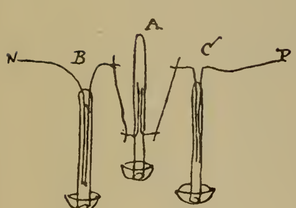
760. Have constructed a new decomposing tube intended to be a volta electrometer. It is about   inches long and   of an inch in diameter. Two platina wires soldered to plate platina poles passed through the side, being hermetically sealed there. So that the tube can be filled with dilute acid or any other solution, inverted in a cup of the same solution: the poles can be connected with the battery, or with other apparatus, and the gas evolved can be collected above out of contact with the Pos. or Neg. pole. No after action of absorption or condensation was found to occur here. Will for the future call this tube A.



761. Arranged this tube with two others, also having each two poles, but these passing in above through the glass in the usual way and therefore always touching the resulting gas more or less; will call these B and C. In B was strong sol. caustic soda—in A dilute S. A. of s.g. 1054—in C dilute solution of nitre. The battery used had four troughs but was partially exhausted. At end of certain time

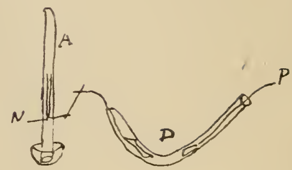
B or caustic soda gave 70·1 vols. of mixed gases.
A or dil. S. A. „ 70·8 „ „
C or sol. nitre only 58·3

Hence a solution of nitre not as sol. sul. soda or sul. acid, etc. etc. etc.

762. A portion of the gas from C or nitre tube, acted on by spongy

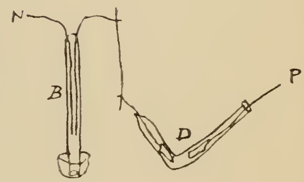
platina, did not leave more than $\frac{1}{20}$ unabsorbed, and that looked like common air. Hence no appearance of excess of oxygen or deficiency of hydrogen, as if the nitric acid had given way.

763. The bubbles evolved during the action in B and C were small and the solutions cloudy. Those in A were larger and the solution bright and clear.



764. Then tried the same solution in a single pole tube to be called D, thus, the S.A. of s.g. 1054 being in A. This was to ascertain if the hydrogen from the nitre were deficient or not. The gas evolved at the N. pole in D in small bubbles, that at the P. pole in larger, and liquid clear there; just the same difference as in solution of alkali and acid. After a while the gas from

N. end of D or nitre solution = 17·25 vols. and
the whole gas from S. A. in A tube = 25·5. These very nearly as 2 : 3, and the hydrogen in both therefore alike, but does not agree with expt. of two poles near each other in nitre solution above.



765. Now put same solution of nitre as above into two tubes B and D to compare action of poles near to and far apart. During the action of battery, the gas in B was evolved from the wires and upper part of pole plates and very little at lower part: as if the electricity discharged almost entirely there. But in D, the gas was evolved at the lower ends of both poles, as near to each other as might be, and very little above; indeed none there, no electricity going so far up the tube from the bend. This indicates good conducting power of nitre solution, but is also equivalent to a concentration or a use of small poles in D and to extension of surface or use of large poles in B. This therefore affect the intensity of the current. The appearances are, as of but little gas from pos. pole in B, but appearances of bubbles, etc. often deceptive.

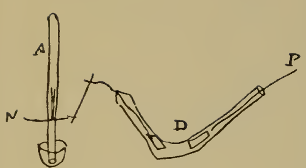
766. The insulation is good, so that all current goes through both tubes. The action of the battery is weak. In tube B the acid and alkali separated can unite again; in tube D not very readily, and the solution is strongly acid at the pos. end. After action over

Gas supposed to be hydrogen from D = 19·6 vols.
Supposed O. and H. from B = 20 vols. only.

Of the 19·6 from D, about 15·4 disappeared by oxygen and spongy platina, but whether the other 4·2 were nitrogen or no I cannot

say: the tube broke. The 20 of gas from B diminished to 3, so that 17 mixed H. and O., of which 5·666 Ox. and 11·333 Hy. The 3 contained more O. than air and seemed to be O. and Nitrogen.

767. These expts. consistent in shewing that in such a tube as B, where poles are large and close together, there is a deficiency of gas as compared to that resulting from decomp. of water. Seems also as if sol. nitre gave different results by same quantity of electricity, when decomposed by large or small plates or by current more or less intense. Must examine this hereafter.



768. Now worked with a Muriate, namely muriate of lime, to see if it would give as much hydrogen as the dilute S. A. or Mur. Acid. Used tubes A and D. In A was the S. A. s.g. 1054. In D a moderately strong solution of Muriate of lime. There was much action on platina at Pos. pole in D and much chlorine evolved; there was also a precipitate, flocculent, formed through the whole solution by the mixture of the different parts. Ultimately the gas from the N. Pole of

D or Mur. lime, supposed to be hydrogen = 42·3 vols.
that from A, *O. and H.* from S. A. = 65 „ ; it should have been 63·45 to compare as O. and H. with the 42·3 of H.

769. Lime had precipitated on the Neg. pole in D; it was removed by M. Acid.

770. Repeated the expt., using a much stronger solution of Mur. lime in D.

The gas from D or Mur. Lime, supposed H. = 49·2
that from A, supposed O. and H. = 72·75, not 73·9
as should be to be proportionate to the 49·2. There was plenty of lime deposited on the N. Pole of D. At Pos. pole apparently all chlorine.

771. Repeated the expt. with same solutions:

Gas from D or Mur. lime = 46·9
Gas from A or sul. acid sol. = 69·0, not 70·35;
lime, etc. at N. pole as before. 18·5 parts of the gas from D with 11·5 oxygen and spongy platina gave diminution of 24·5, of which 16·33 the hydrogen; the other 2·17 appeared to be nitrogen or air.

772. Fearing the Mur. lime used might contain a little nitrate,

I made a very strong solution from pure Mur. acid and Marble, and used it in that very strong state, repeating the last experiment in every other particular. Much chlorine was evolved as gas from the P. pole of D, less being apparently dissolved, and there did not appear to be any thing like so much action on the platina as before. Finally

Gas from D or Mur. lime, supposed hydrogen = 47·2 vols.

Gas from A or dil. S. A., „ O. and H. = 66·75, not 70·8

773. I suspect that some process of solution of the gas in particular cases has much to do with these small and irregular differences. Here the sol. Mur. lime dissolved little or no chlorine, and therefore probably no hydrogen. 20 vols. of the gas from D tube with 12 Ox. diminished by spongy platina to 4·6—indicating 18·26 in the 20 as hydrogen. I am doubtful whether the 1·74 is nitrogen or air or some other gas, or whether there was not enough oxygen to ensure complete condensation of all the hydrogen.

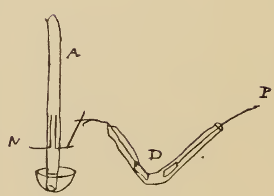
774. Now used a weaker solution of the same muriate of lime, in proportion of 1 of the strong and 3·5 of distilled water. This time the chlorine at P. pole in D appeared to dissolve.

Finally, Gas from D or Mur. lime (H.) 47·6 vols.

„ „ A or Sul. acid (O. and H.) 69·25, instead of 71·4.

775. Began to think that the form and conditions of the decomposing tubes might have something to do with this constant small difference always or almost always in the same direction, and therefore dismissed the Mur. of lime and used only the dilute S. A. s.g. 1054, putting that into both tubes as in the figure. After a time the gas in D, Hydrogen = 51·6

gas in A, (O. and H.) = 75·4 instead of 77·4.



Yet in this case the same acid used in both and the same current.

776. Repeated the expt. with S. A. s.g. 1054 in both tubes.

D gave gas (H.) = 47·4

A „ „ (O. and H.) = 67·65 instead of 71·1.

Tried these gases: 18 vols. of that from D + 18 oxygen diminished by platina to 12, indicating only 16 of the 18 to be hydrogen—24 vols. of the gas from A + 12 of oxygen diminished by platina to 13·6. Hence only 20·4 had disappeared as O. and H. of the 24.

777. Hence apparent absorption of some of the gas evolved by the voltaic action, or else evolution of some gas as air, etc. from the acid during the action, independant of that due to the decomposition.

778. On heating some of the Sul. acid diluted in a flask it did not give any signs of air dissolved—as water would do. Transferred some gas to and fro over the trough but did not perceive any diminution by absorption or other change of volume produced by the mechanical agitation and transference, etc. etc.

779. It rather seems as if the Pos. pole had caused absorption in some way in the double tube. It is not unlikely that it may have a power like that it has in gases, and may make the oxygen evolved combine with the water, forming something like oxy water (Mr Pearsall has shewn that it bleaches) and it does not seem impossible but that the N. Pole shall have some contrary action rather tending to evolve gas. But this, by the bye, does not seem to occur in tube A; i.e. no compensation for action of P. Pole occurs. On the *whole*, there is deficiency.

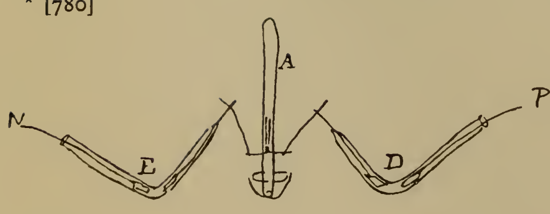
780. Now put dilute S. A. s.g. 1054 thus*, so as to collect gas both at a P. and at a N. pole, as well as in A from both poles near together. After a while the battery was stopped and the volumes of gas examined.

The gas in E—(Ox.)	22 vols.
„ „ A—(Ox. and Hy.)	68 vols.
„ „ D—(H.)	48·1 vols.

Results here remarkable—first A is to D as before; it has 68 vols. only instead of 72·15. But then as compared to E it has *too much gas*, for it is 68 instead of 66, and E and D differ still more; instead of being as 1 : 2, they are as 22 : 48.

781†. It seems almost certain here that P. pole has some dissolving or combining action on gas evolved from it, as respects the solution; and if this power can be retained, as that which it has over gases can, then by making that pole Pos. which before was Neg. may compensate in part for the effect. Changed therefore the two tubes D and E for each other, so that the collecting poles should be the reverse of what they were, but used the same acid as before, s.g. 1054, in all. The battery was now very weak and the time of

* [780]



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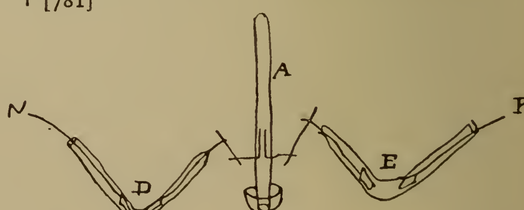

action therefore long, but after a sufficient quantity of gas was evolved, the relative portions were measured.

The gas in D–(Ox.)	now	= 16·6
,, ,, A–(O. and H.)	,,	= 49·2
,, ,, E–(H.)	,,	= 33·5

Here A is to E as before, but not so far off; it is as 49·2 (instead of 50·25) to 33·5–E therefore seems to have given less gas than on former occasions–(A has not been changed in any circumstance). The ratio of D to E is still more remarkable, for they are very nearly as 1 : 2, as they ought to be. This looks very like a peculiar action conferred on the platina by its being made either P. or N. Pole, but what are its characters after being made N. Pole, I am yet not clear about, or its relation to ordinary platina.

782*. The tube A has all along been used in one position and direction, one of its poles having been constantly Pos. and the other as constantly negative. *Now turned it round half way*, so as to make that which had been P. now Neg., etc. etc. and again changed D and E for each other.

Gas in E–(O.)	= 21·5
,, ,, A–(O. and H.)	= 65·8
,, ,, D–(H.)	= 44·4

The battery was very weak and the action long.

Now A is to D as 65·8 (instead of 66·6) to 44·4, so that it is not far away; not so far as in former cases, and it is almost exactly equal to D and E together–but they differ, the P. pole being deficient or the N. Pole in excess.

783. Put two poles of battery near each other in solution of acetate of lead. The lead on N. Pole continued to increase until it *completed the communication* metallically. Much per oxide of lead went to P. pole. Hence mutual relation and action of particles in decomposing solution.

784. It was easy to obtain crystals of lead so thin as to be quite transparent, yet metallic and brilliant in a proper light.

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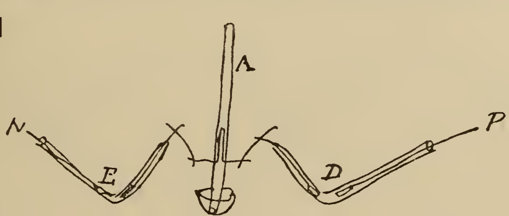
785. Have made some new tubes for the purpose of obtaining the O. and H. evolved from the same decomposing solution, and ascertaining real volumes, etc. at the opposite poles. There were

* [782]



six of them. The pole was in each a platina wire terminated by a platina plate soldered to it by gold. The wire was sealed into the glass at the top of the tube and the plate came within $\frac{1}{4}$ inch of the open end: as near as it could, to lose no gas by currents of fluid carrying bubbles down and out of the tube.

786. These were then used in three successive portions of an arrangement acted on at once by the voltaic battery. The battery consisted of four troughs. The tubes were put into basins containing the same solution as that used in the tube and the mouths placed near to each other, so as to facilitate the transfer of gas. The following may represent a section of tubes, etc. etc.*

The troughs had been newly charged. The notes shall represent the results in the same order in which they stand upon the paper above* and the tubes may for the present be identified by the Nos. 1, 2, 3, 4, 5, 6, so that any ulterior effect due to a particular tube may be ascertained.

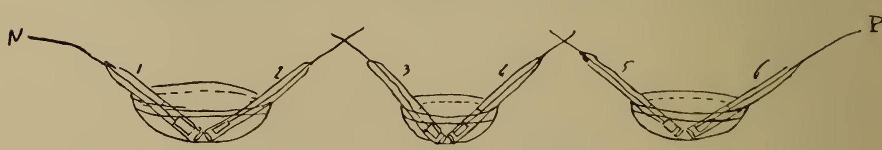
787. In the first experiment, dilute Sul. acid of S. G. 1232 occupied the first two tubes and basin—dilute sul. acid s.g. 1054, which has been so frequently used, occupied the 3 and 4 tubes and basin, and strong solution of caustic potassa occupied the 5 and 6 tubes and basin. After action of the battery the results were as follows:

S. A. sol. 1232	S. A. sol. 1054	Potash solution.
tube 1, 34·5 vols. hy.	tube 3, 33·5 vols.	tube 5, 33·3 vols.
„ 2, 17·4 „ Ox.	„ 4, 15·9 only	„ 6, 16·7 vols.

788. In the strong S. A. and in potash the hydrogen is as nearly as may be expected double the Oxygen, but the Potash has given less altogether than the acid. In the S. A. 1054, as in the former instance the oxygen is much less than half the hydrogen. The hydrogen itself is less than the hydrogen of S. A. 1232, and it is probable that that is the nearest to the truth, but whether the whole deficiency in the others is due to solution of the gases separated or that transfer of the acid and alkali in those cases has occupied part of the electro chemical power of the current, I cannot yet say.

789. Repeated the expt., wiping the tubes carefully on the outside least any electricity should pass that way, but not likely. I also

* [786]



changed the places of the strong and weak sulphuric acid to see if that did any thing.

Sul. Acid 1054	Sul. Acid 1232	Sol. Potash
Tube 3 gave 39·5 vols. H.	Tube 1 gave 40·4 vols. H.	Tube 5 gave 38·8 vols. H.
„ 4 „ 18·5 „ O.	„ 2 „ 20·4 „ O.	„ 6 „ 19·6 vols. O.

Here results very nearly as before and just in the same order. Hence changing place does nothing, nor the wiping of tubes, etc.

790. The oxygen gas evolved from the acid smell[s] very powerfully like Electricity, or phosphorus or nitric acid, etc. but the hydrogen not at all. Nor does the oxygen at the P. Pole in Alkali smell. Can nitric acid have been formed here? It will account in part for diminution of oxygen and also for smell not occurring in alkali where the disappear. of oxygen considerable. Perhaps using ammoniacal salt or solution or little amm. in acid might give much more disappearance, and perhaps boiled water not give disappearance. Must try that.

791. Now changed the tubes only of the strong and weak sulphuric acid. It so happens that as the solution of strong acid has but just been made, it is hardly cold and air may have been expelled from it and not yet taken up again.

S. A. 1054	S. A. 1232	Sol. potash
Tube 1 gave 39·1 vols. H.	Tube 3 gave 39·9 H.	Tube 5 gave 39·4 H.
„ 2 „ 18·3 „ O.	„ 4 „ 19·7 O.	„ 6 „ 18·9 O.

792. In this expt. I placed the tubes so that the plane of the different poles were vertical, and in that way the gas separated better and escaped to the upper part of the tube and no small bubbles were carried out by the descending currents. It was a great improvement—especially in the potash solution where the bubbles are very minute—easily escape, easily dissolve.

793. The results here nearly the same as before. The potash not much behind the strong S. A. in the hydrogen, but the oxygen both of it and weak acid minus.

794. Oxygen smells of electricity.

795. The changing tubes has made no difference. Repeated the experiment without any alteration as to place of solutions, but changed tubes for a final trial of that circumstance.

S. A. 1054	S. A. 1232	Sol. Potash.
Tube 5—38·2 vols. H.	Tube 3—38·7 H.	Tube 1—38·4 vols. H.
6—17·4 „ O.	4—19·4 O.	2—18·4 „ O.

796. Changing tubes has done nothing. Results are as before. Strong S. A. looks well: weak S. A. deficient. Sol. Potash near in the H. but short in the Oxygen.

797. Now dismissed the weak S. A. 1054, and made a very strong one of 8 oz. measured Oil Vitriol and 4 oz. measured of water. This when cooled proved very oily indeed and was more like oil in appearance than the original stronger acid. It is probably near the freezing acid in composition and is another distinction of that state of combination of sulphuric acid and water.

Strongest S. A.	S. A. 1232	Strong sol. pure M. A.
Tube 5–42·0 H.	Tube 3–40·5	Tube 1–40·5–H.
6–11·8 O.	4–20·1	2– 0·0–

The strongest S. A. gave more H. than any, the other S. A. and M. A. are alike; and the ox. of the weaker S. A. is very nearly in right proportion. The product at the P. Pole in M. A. was chlorine. But the curious matter is in the Strongest Sul. Acid–the hydrogen was pure; it contained no sulphur. The oxygen was also pure. I do not see how to account for the effect except by supposing that Sul. acid has considerable solvent power over oxygen or by supposing a new acid of sulphur, containing more oxygen than the sulphuric.

798. Must try bleaching powers of acid oxygenized at the P. pole of voltaic battery.

799. Repeated the expt.

S. A. 1232	Strongest S. A.	M. A. solution.
H. tube–39 vols.	H. tube–39·5 vols.	H. tube–38·7 vols.
O. „ –19·6 „	O. tube–about same as last, but lost the result; it was as before very deficient.	C „ – 0·0 gas absorbed.

Results therefore agree with the former, and it would seem that in S. A. 1232 there is still some solution of gas evolved by decomposition of water.

800. Have made another Sul. acid,

$$\left.\begin{array}{l}\text{8 measured oz. oil vitriol}\\ \text{8 Do. Do. distilled water}\end{array}\right\}\begin{array}{l}\text{s.g.}\\ \text{1495.}\end{array}$$

801. Think it will be very important to have a new relation of bodies under the term *electro-chemical equivalents*–tabulated. Very important as to decomposing powers of the pile–as to the true

expression of equivalent numbers and as to nature of chemical affinity and its relation to electrical states and powers.

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802. If in decomposing a solution of lead, as acetate or other salt, the same quantity of lead is produced whether per oxide or other substance is formed or not at P. Pole: will then shew that the action of separation is evidently not at the poles, but internally: and that the final state is of no consequence.

803. The per oxide of lead formed there is most probably a result of the peculiar combining power possessed by the platina which has been made positive, and is probably no direct consequence of electro chemical decomposition.

804. The same thing will be shewn if hydrogen evolved at the N. Pole remains constant when the oxygen at the P. Pole is either set free, or combined as with lead or zinc pole—try this.

805. But these effects will require to be considered in obtaining the true expression of electro chemical equivalents.

806. Then must be some effect as to the production of new bodies internally, as of nitrous acid in N. A. decomposition. Perhaps may ascertain what are the *true acting evolving bodies* concerned in producing under the influence of the current decomposition; as distinguished from final bodies, often depending upon secondary chemical action of the pole.

807. *Cases of recombination of Elements.* Water under pressure. This effect probably a common one, and the cause why sometimes decomposition is not finally effected. As for instance in Sulphurous acid and other liquefied gasious compounds which do not decompose.

808. May be the case also with chlorine, iodine, etc. etc., metal, etc. etc. and the reason why their elements do not separate.

809. Must ascertain the effect of the two solutions at the two poles on different bodies. Should be oxy. solution at the P. Pole and hydro solution at the N. pole. Action of the latter especially in reducing bodies.

810. In case where three pr. plates did not decompose (   ) sensibly, perhaps all the O. and H. dissolved, and by mixture re-

combining elsewhere; then a parallel case to water under pressure as before noticed, and also to rest of such phenomena.

811. Then the phenomena would not shew that the intensity was too small—but be otherwise accounted for.

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812. Have continued experiments like those in form of the 23rd instant. The battery had been much used and was considerably exhausted. The platina poles in tubes were placed edgeways, etc. etc.

Sul. Acid s.g. 1495.	Sul. acid s.g. 1250.	Strong sol. pure Amm. rendered conducting by sulphate Amm. dissolved.
Neg. tube—33·4 vols. H.	N. tube—33·3 H.	N. tube—33·1 vols. H.
Pos. „ —16·6 „ O.	P. „ —16·55 O.	P. „ — 8·65 „ N.?

There was good decomposition in all; hydrogen seemed most in Amm. N. tube, but this due to Amm. gas carried up at time. It is remarkable that not more hydrogen from the Ammonia than from water, for these being alike, the contents of the P. tubes bear no relation to each other consistent with the present scale of proportional numbers.

813. Repeated the expt.

S. A. s.g. 1495	S. A. s.g. 1250	Sol. Amm, etc. etc.
N. tube—37·9 H.	N. tube—37·7 H.	N. tube—37·3 H.
P. „ —19·0 O.	P. „ —19·0 O.	P. „ — 8·8 N.?

As before except that the Amm. product at P. pole not now $\frac{1}{4}$ of the hydrogen—indeed, neither of them are right proportionals as Nitrogen for the hydrogen evolved.

814. Now used the acid of s.g. 1495 in two places, with one portion using poles such as those employed all along, that is platina plates, but with the other using tubes containing a wire pole, that they might expose less surface at which the oxygen or hydrogen could be dissolved by the water. The Ammonia was diluted with its bulk of water.

S. A. s.g. 1495 plate poles	S. A. s.g. 1495 Wire pole	Sol. Amm. 1 vol. Water 1 vol. } plate poles
N. tube—39·9 H.	N. tube—40· H.	N. tube—39·2 H.
P. „ —20· O.	P. „ —20·4 O.	P. „ — 7· N. and O.

815. The S. A. s.g. 1495 results have been very good all along, but the wires poles are perhaps a little better than the plates—but they retard the current and require longer action of battery. The ammonia results are deficient still at P. Pole. It is probable that both Amm. and water may be decomposed there and the peculiar power of the P. pole cause part of the N. and O. to unite and form Nitric acid. This would account for the deficiency. Try the gas therefore. Now taking the mixed gases from the P. Pole in Ammonia of the two last experiments—

10 Vols. mixed with

10 vols. of Hydrogen and acted on by heated spongy platina gave 18·3 vols.; hence

diminution 1·7, of which $\frac{1}{3}$ or 0·566 is oxygen; the rest of the 10 vols. appeared to be Nitrogen. Hence both O. and N. are evolved by the electro chemical action and it is not wonderful that they should have partly combined to form soluble compounds and diminished the volumes of gases.

816. Must try the gases evolved by the strongest Amm. Solution.

817. It is remarkable to notice the constant relation of the current to the hydrogen evolved. As this is the lightest and most elementary of substances in its character, combining in the smallest quantity, the circumstance of constant evolution may perhaps indicate that it is more simple in its nature than other acknowledged elements, and also most natural standard.

818. Consider whether circumstance can depend upon the decomposition of water in this case, the Oxygen evolved at the P. pole decomposing Ammonia there and combining both with its hydrogen and nitrogen. In that case Ammonia not decomposed electrically by current but by ordinary chemical affinity: it looks more likely than the above strange relation.

819. If Ammonia decomposed by current, then it would be very remarkable that, whether it be the water or the ammonia or both that is decomposed, the final result relative to the hydrogen is the same, the proportion of oxygen or nitrogen being indifferent; but I feel that it is more likely the ammonia suffers by a secondary action, and its giving no conducting power to water when dissolved in it indicates this.

820. May it not then follow that bodies which do give conducting

power are decomposed? But then how stands dilute Sul. acid? Mur. acid may be either one way or the other.

821. The hydrogen seems to be an excellent indicating element, for there is no other element at the N. pole to mystify it.

822. Now worked with a new battery—used the S. A. s.g. 1495 as the check or indicator of electricity, for it seems very constant and good, and associated other bodies with it to ascertain their relations.

S. A. s.g. 1495	Sol. Salt nearly saturated.	Strong sol. Mur. lime.
N. tube, 27·7 vols. H.	N. tube 27·5 H.	N. tube—27·5 H.
P. „ 13·8 „ O.	P. „ 0·33 Azote or air	P. „ — 0·3 N. or air

In the salt and Mur. lime, chlorine evolved at P. pole—it acted on the platina and gold solder—part also dissolved, forming a very bleaching solution; and a little escaped in gas which disappeared on agitation in water, leaving the small residue of air from the liquors mentioned above. There was plenty of lime at N. pole in Mur. lime. No Oxygen at P. poles of salt or Mur. lime, and the hydrogen so near as to be considered the same in all.

823.

S. A. s.g. 1495	Strong Sol. Sub carb. pot.	Strong Sol. Mur. Am.
N. tube—37 vols. H.	N. tube 36·8. H.	N. tube—37·0
P. „ —16·9 ? O.	P. „ only 7·8 O. Oxygen here very short but good in quality—there was a deposit of something looking like lead on N. pole.	P. „ — 1·5 nitrogen. Chlorine evolved here acted on the gold; it also formed azotane, and that evolved, as is usual with it, the Azote.

The hydrogen agrees well in all and shews utility of that as the indicating element. Query, what has become of the Oxygen in the Carb. potash solution.

824. Repeated the last expt.

S. A. 1495	Sub carb. pot.	Mur. Am.
N. tube—33·8 H.	N. tube —32·6 H.	N. tube—32·8 H.
P. „ —15·8 O.	P „ only 4·8 O.	P. tube— 3·0 N.

Here the oxygen in the Alkali very deficient; must be a ready combination in some way. The hydrogen agrees in all pretty well.

The N. from Mur. Amm. is increased because, having used the same solution, the previous state of solution of chlorine or of Azotane make a more ready evoluter by reaction upon this salt, and a quicker spontaneous decomposition of the azotane dissolved.

825. Must change the P. poles of the first and second parts of the experiments for the N. Poles, to ascertain whether their peculiar power has helped in the great deficiency of O. in the Carb. potash, and the deficiency also in the S. A., which has not occurred to anything like this extent before.

826.

S. A. 1495	Sub carb. pot.	Sol. Common salt with a zinc Pos. pole.
N. Pole–35·4 H.	N. pole–35 H.	N. pole–34·5
P. „ –17·0 O.	P. „ – 7·5 O.	P. „ – 0·0 no gas evolved or dissolved–all combined with zinc as chloride
What had been P. pole in former expts. now made N.	The P. pole here in former expts. now made N. pole, etc. etc.	

Still the acid gives too little oxygen. I think it is too strong now it begins to warm and that like very strong acid oxygen is dissolved or combined by it. Must make a weaker acid. Changing the poles may perhaps have done a little but leaves much uncorrected. In the alkali and sol. salt, there is precipitation on the neg. pole–in the first of lead apparently, in the second of zinc probably. These may account for the diminution of hydrogen in some degree, i.e. if metal can replace hydrogen.

827. On neutralizing some of the Carb. pot. by N. A., etc. etc. I found it contained some muriate, and this may account for the disappearance of gas there at P. Pole, i.e. probably chlorine has been set free there.

Weaker S. A. 1250.	Sat. sol. sul. soda.	Sat. sol. sul. soda. 1 vol. Water 5 vols.
N. tube–24·0 H.	N. tube–24 H.	N. tube–24·0 H.
P. „ –11·0 O.	P. „ –14 O.	P. „ –11 O.

Must repeat this expt. and examine Oxygen at strong sul. soda. O. in S. A. very deficient–also in sul. soda.

828. Bodies at the poles are of two classes. Those simply evolved by the electro chemical action of the current and those produced there by their action or combination either with the pole or the

bodies in solution. Sometimes one, as when zinc pole used, sometimes other as when per oxide of lead formed or in case of decomposition of ammonia.

829. Consideration of the substances produced and of their quantities will often decide whether they are in the first class or in the second—and consequently often decide what is the body which has really suffered Electro chemical decomposition.

830. On cause of salts giving conducting and decomposing power, refer to 410 of the researches, fourth series.

831. On the connection of conduction and decomposition, look back to fourth series of researches.

832. References as to action of interposed plates:

Davy, Phil. Trans., 1826, p. 413.

De la Rive, Ann. de Chimie, xxviii, 190.

833. Peculiar property of Pos. pole. See Davy. Phil. Trans., 1826, p. 412, 413.

834. Conduction. Davy on. Phil. Trans., 1826, p. 428.

835. Davy on Herschell on Motions of mercury under fluids. Phil. Tr., 1826, p. 416.

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836. Continued expts. on simultaneous decomposition: first tried the three sul. acids.

Sul. acid s.g. 1495	Sul. acid s.g. 1336	Sul. acid s.g. 1232
N. tube—30·4 H.	N. tube—30·0	N. tube—30·4
P. „ —14·4 Ox.	P. „ —15·4	P. „ —15·3

Here all the hydrogens nearly alike, very nearly. The strongest S. A. deficient in Oxygen, the other two very nearly alike, and both rather more than half hydrogen. Errors from mixed air probably and absorption, but S. A. of s.g. 1336 seems very good.

837. Now used S. A. s.g. 1336 in all three parts of expt., but in one used the volta-electrometer with plates, in another used one with wires only but otherwise constructed in the same way, and in the third used two separate tubes.

S. A. S.g. 1336

Gas 55·3 vols.

S. A. s.g. 1336

Gas 55·3 vols.

S. A. s.g. 1336

N. tube—36

P. tube—18·4

54·4

The two first alike, so that both apparently equally good. The oxygen in third very nearly indeed $\frac{1}{3}$ of whole volume in others, but the hydrogen a little deficient.

838. Repeated the last expt.

S. A. 1336. Plates	S. A. 1336. wires	S. A. 1336. separate tubes
Gas 74·3 vols.	Gas 73·25	N. tube—49·25
		P. tube—24·4
		73·65

Now the separate tubes are between the other two, and there are other little differences, but the coincidences are very mark[ed]. In this expt. the two outside parts were insulated, but there seems to be no additional effect produced.

839. Now reverted to expt. on solutions of Sul. Soda.

S. A. s.g. 1336 Plate Volta eudiometer	Sat. sol. sul. soda in separate tube	Sat. sol. sul. soda 1 vol. Water 5 vols. in separate tubes.
Gas—27·0 vols.	N. tube—18·4	N. tube—18·45
	P. tube— 9·4	P. tube— 9·4
	whole 27·8	27·85

Here results of Sul. soda very close; no difference in strong or weak, both beat the S. A. The weak sul. soda conducted but badly: the action was therefore longer and probably more gas dissolved.

840. The oxygen smelt of Electricity.

841. *Sulphuric acid diluted*—Sol. *sulphurous acid* and *strong Nitric acid*. The sol. sulphurous acid contained sulphuric acid. There was very little gas at either pole; at the N. Pole much sulphur rose in a milky stream and liquor there soon turbid. As to the nitric acid, the oxygen only was collected; there was indeed no gas at the neg. pole, only nitrous acid which dissolved.

S. A. s.g. 1336 in plate volta electrometer.	Sol. Sulphurous acid in two tubes	Strong nitric acid in [tube]*
whole gas 53·4 vols.	P. pole—4·35 was oxygen	P. pole—17·1 oxygen
	N. pole—4 was hydrogen	N. pole—nitrous acid.

*

842. The effects on Sulphurous acid are I believe secondary at both poles, i.e. that water only has been electrically decomposed,

but that the Oxygen has at the P. pole converted sulphurous into sulphuric acid and the Hydrogen at the N. pole reduced part of the Suls. acid. The gas there was pure hydrogen, not sul. hydrogen.

843. I think also the effects at the N. Pole in Nitric acid are secondary, i.e. that the hydrogen evolved decomposes nitric acid, reducing it to nitrous acid. The oxygen seems to be a pure electrical result; it is very nearly $\frac{1}{3}$ the vol. of gas in the volta eudiometer, i.e. equal to the oxygen there. This oxygen *did not smell* of Electricity.

844. *Sul. acid*, 1336—*Strong solution of Acetate Potassa—Strong Nitric acid.*

Here the nitric acid was repeated to compare with the above.

S. A. 1336 Plate volta electrometer	Acetate Potassa strong.	Strong N. Acid.
Gas 37 vols.	P. pole—34·5 vols. gas N. pole—24·6	P. pole 12·3 oxy. N. – Nitrous acid.

The nitric acid very good. The oxygen as nearly as possible $\frac{1}{3}$ the mixed O. and H. of first tube. The acetate curious. The hydrogen is exactly as if from water, and the gas at the P. pole is probably the result of the action of oxygen evolved on the acetic acid, in fact a secondary result. The 34·5 vols. gave 12·1 of carb. acid and 16·4 of carb. oxide, but probably some of the Carb. acid has been dissolved.

845. Repeated the acetate of potash expt.

S. A. s.g. 1336 in plate volta electrom.	Strong sol. acetate potash.
Gas 50·4 vols.	N. pole—33·4 H. being as nearly as possible $\frac{2}{3}$ of first. P. pole—42·4 of which 23 Carb. oxide 19·4 Carb. acid.

It is probable that if none were dissolved there would be an equal bulk of carbonic oxide and carbonic acid. It is also most probable that the results at the P. pole are of a secondary nature, but I do not yet see how it is that so great a bulk of oxy. compounds is evolved there, as the O. and H. in acetic acid are in the proportions to form water.

There seems to be only pure hydrogen at the N. pole, for
22 vols. of that gas with
17 „ oxygen, diminished by platina to
39
8·6 hence
30·4 was the diminution, of which 2/3 = 20·3, so that the 22 could have contained no comb. hydrogen, or more oxygen would have disappeared.

846. I think the results at the P. Pole are secondary or else surely carbon should have appeared at the N. pole, where it does not appear. But I must look at the unabsorbed portion of the gas at the P. Pole to see whether it is not a carburetted hydrogen—it seems very likely that it may be. Must make out where so much gas comes from.

847. Made another expt. with diluted solution of acetate of potash; it was 1 vol. of the strong solution + 3 vols. distilled water.

S.A. s.g. 1336 plate Volta electrometer	Dilute sol. acetate potassa
Gas 56·3 vols.	N. pole—37 vols. H. P. pole—25 vols., consisting of 11·4 vols. absorbed by potassa and 13·6 unabsorbed, and supposed to be C. oxide—but on mixing this with oxygen to burn it in an open tube, it appeared to me to want much more oxygen than C. oxide. Possibly a carburetted hydrogen.

848. Must try a dilute acetic acid. It is very possible that with Tartaric acid, etc. etc. etc., this mode of decomposition at P. Pole may be rendered available in analysis or in obtaining views of ultimate composition. Benzoates, etc. etc. etc.

849. Tried the hydrocyanic acid of Apothecaries Hall, using a tube in which the results at the N. Pole could be collected.

S. A. s.g. 1336	Hydro cyanic acid
in plate volta electrometer	N. Pole—6·6 vols.
gas—10·1 vols.	was hydrogen

Conducted very badly and could obtain very little gas in a reasonable time. This looks as if it did not decompose by voltaic battery, or like M.A. it probably would have increased conducting power.

The 6·6 of hydrogen is nearly $\frac{2}{3}$ of the 10·1, so that it looks like the hydrogen of water, but as with Mur. acid it might be the hydrogen of the hydrocyanic acid. Gas was evolved at the P. Pole, and therefore the expt. was repeated, collecting the gas at that pole only.

850.

S. A. s.g. 1336 plate volta electrom.	Hydrocyanic acid solution
Gas, 12·4 vols.	P. Pole, 2·4 vols. gas. This gas was pure oxygen, but it is much less than the oxygen in 12·4 vols. at the other pole. There has probably been a secondary action at this pole.

851. It seems probable that only those elements which can combine with each other chemically, expell or influence each other to decomposition of their joint product under the influence of the circuit. Thus hydrogen conjointly with oxygen, chlorine, etc. etc. is determined one way, they the other: acids with bases are determined in opposite directions, but hydrogen has no relation to an acid or oxygen to a base in making each other proceed in opposite directions. When therefore metallic solutions are decomposed, the metals are evolved not by the current of electricity, but by the hydrogen evolved at the N. Pole, i.e. the oxide of the metal is determined to the N. Pole and is there reduced by the hydrogen also evolved or determined there. Hence the cause of the disappearance of the hydrogen and the appearance of the metal.

852. Hence it will probably follow that in these cases the metal is an equivalent of the hydrogen, because it is produced chemically by the hydrogen, and therefore such effects *will not* PROVE the equivalent character of the products of true electrochemical decomposition. But they may and will probably agree with other proofs of it.

853. There may be many valuable secondary effects thus producible.

854. Perhaps even Potassium in Davy's decomposition a secondary effect. As Potash a decomposible substance when fuzed however, the result is most probably one of a mixed nature.

855. Two modes of decomposition are therefore involved in the action of the pile.

One by the direct action of the current.

Other by the oxygen or hydrogen or other element evolved, acting in a secondary manner.

856. Also a mode of effecting *combination*, i.e. matter evolved with matter of solution or of pole present. This is the mode Becquerel has used. Quar. Jour., xxiv, p. 462; xxviii, 409.

857. When water is present, probably all the results besides oxygen and hydrogen which are elementary, and many that are compound, are secondary.

858. Of course, acid and alkali from a salt are not secondary, or at least do not appear to me to be so at present.

859. Hence cause of disappearance of oxygen and hydrogen; and when hydrogen disappears, perhaps it always INDICATE secondary action. The same most likely true of oxygen.

860. Now know why in proto chloride of tin per chloride is produced at the Pos. pole. Is a case of secondary action.

861. May in many cases use a pole of charcoal or of the carbon from the inside of gas retorts as the P. Pole in melted chlorides, to avoid the formation of metallic chloride from the platina pole.

862. Perhaps fuzed nitre will be a good salt to compare by current with decomposition of water. Or fuzed chloride of lead or tin.

863. Alloying or amalgamation of Pos. Pole. Davy.

864. How will proportion of decomposition in Sulphuret of Silver agree with that of water.

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865. Have been observing circumstances of decomposition in strong Mur. acid using a silver P. pole. In the first place all the chlorine unites to the silver to form chloride—none is set free—the liquor has no bleaching powers. A platm. Pos. pole bleached instantly.

866. In the next place, decomposition went on regularly as if the deposit of chloride were no impediment, for it adhered to the silver plate, and being a solid ought to have prevented action: but on examination it was found that over a large part of the surface the chloride had formed a continuous dark coat and there no further action or corrosion went on—in other parts it scaled up

or white loose tufts formed, thrgh. which the Mur. acid penetrated and here the action continued to proceed. So that where the chloride was continuous there it did insulate and prevent further action.

867. A solution of salt was decomposed using a Zinc positive pole; there was not the slightest appearance of bleaching.

868. Tried bleaching action of P. Pole platina in dilute Sul. acid. The acid was coloured by a little sul. indigo, and then carefully tested to ascertain the absence of chlorine or muriates, etc. etc. Only two troughs were used and those rather weak. But in 20′ there was not a certain indication of decoloration in the glass containing the P. and N. Poles. It may have happened, for there was a little difference, but it is doubtful. Must try stronger battery and each pole apart. Of course a very little chlorine or muriate shews bleaching at once.

869. The independance of the quantity of gases evolved in dilute sul. acid or in Mur. Acid of the nature of the poles, whether platina, zinc or other matter, shews most decidedly that the true electrochemical decomposition is quite independant of the attraction, etc. etc. of the poles, for changing their nature makes no change in the quantity of electro-decomposition. This quite agrees with my theory.

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870. Sulphuret of silver perhaps warms, etc. by current because of decomposition.

871. When sulrt. silver decomposes, is the decomposition at all equivalent in extent to the decomposition of Sul. acid solution or other bodies? I suspect not. It perhaps stands intermediately between metals which conduct without decomposing and those bodies which will not conduct without simultaneous decomposition. It may be important in determining relation of conduction to decomposition.

872. Will carbon (from retort) and platina make voltaic circuit—in fuzed chlorides, solution, etc. etc.?

873. Will carbon protect copper in sea water? Or will dry or underpolled copper resist action of sea water?

873½*. Expts. on reduction of acetate of lead.

S. A. 1336 in plate Volta electrometer. Gas 17·6 vols.	Acetate of lead in a tube and basin, the tube pole being made positive for the collection of gas, etc.

The decomposition was very slow, so that I had to stop the expt. before much electricity had passed; the gas in the volta-electrometer = 17·6 vols. There was very little gas at P. pole in acetate of lead, but very fine colours *metallo-chrome* from deposition of per oxide of lead on pole. At N. Pole the lead when washed, collected and dried, weighed 0·4 of a grain.

Now vols. make a cubical inch, therefore the 17·6 vols. contained of c: i: of hydrogen, which would weigh of gr., and that is to 0·4 as 1 : ; the equivalent number of lead is

874. *Chloride of lime*—a filtered solution of bleaching powder.

S. A. 1336 in plate Volta Electrometer	Sol. Chloride of lime strong
gas, 47·5 vols.	P. Pole— 7·5 vols. pure oxygen.
	N. Pole—12 vols. pure hydrogen.

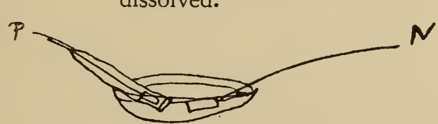
The proportion at the two poles in bleaching liquor are not to each other as in water, nor are they in any proportion to the whole vol. in Volta Electrometer. There must have been secondary action at both poles, and as Mur. lime would be present, that has affected results and made them very irregular.

875. *Strong oil of vitriol* acted on by four weak troughs: there was no decomposition of the acid, only of the water. No sulphur or sulphuretted hydrogen, but pure hydrogen at the N. Pole and pure oxygen at the P. pole. If strong battery will decompose, shews effect of intensity.

876. Tried to evolve acid at N. Pole where metal of a salt reduced by hydrogen evolved. Acetate of lead is not neutral. Sub acetate of lead, by weak battery, did not shew acid at N. Pole. Nitrate of lead is always acid.

877. Now tried Sols. Ferro pruss. pot. and Iodide Potassa.

S. A. 1336 in plate Volta Electrometer.	Sat. Sol. Ferro pruss. pot.	Strong sol. Iodide Pm.
	N. Pole—27·5 H.	N. Pole—25·8 H.
whole gas, 41·6 vols.	P. „ —very little gas; only a few bubbles at first—none afterwards—the ferro pruss. acid dissolved.	P. „ —no gas; only iodine which dissolved in liquor.

* [873½] 

In these cases the hydrogen from ferro pruss. pot. quite right in proportion and that from Iodide Pm. not very deficient. As the iodine dissolved and diffused itself, it is possible that part of the hydrogen evolved at the N. pole combined at the moment with iodine again and caused the deficiency.

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878. Motions of mercury. See Quar. Jour., xxv, 216.

879. De la Rive, Conductibility conferred on or by water. Quar. Jour., xxvii, 407.
" " " also by Iodine, Bromine, etc. xxiv, 465.
See Kemp on Suls. acid. Quar. Jourl., 1831, p. 613.

880. Conducting power of liquefied condensed gas. Kemp. Quar. Jourl., 1831, p. 613.

881. Fusinieri on transfer of ponderable matter by spark. Gior. di Fisica., viii, 450.

882. Experimented on the action of platina poles on Gases. Had a new charge of battery 40 pr. Plates 4 inches square. Prepared several poles consisting of pieces of platina foil soldered with gold on to platina wires; the pieces of platina were about half an inch wide and $2\frac{1}{2}$ long. A number of tubes were also prepared containing from $1\frac{1}{2}$ to 2 cubic inches, closed at one end. In the experiments to be described, these were filled with a mixture of 2 vols. H. + 1 vol. Ox., and then the platina pole or plate introduced into the gas through the water of the trough and the whole set aside in a small jar for action and observation.

883. These poles or plates were made pos. whilst a very large plate of platina was made Neg. in a jar of the Sul. acid, s.g. 1336. They were retained there only one minute, after which they were withdrawn, washed in water and introduced into 3 tubes previously filled with O. and H., at the pneumatic trough. They instantly caused condensation, the gas evidently diminishing and in periods from 6 to 8 minutes nearly the whole had disappeared in all the jars or tubes.

884. One of these poles was then transferred to a fresh mixture—there was action upon it but the effect was slow by comparison to what it had been.

885. Three fresh poles were now made Pos. in the S.A. 1336 for five minutes, and then washed and put into mixed gases O. and H. They acted more rapidly than the former, considerably so. The diminution went on beautifully and the upper ends of the tubes became quite warm. These poles each of them in the space of one minute made about $1\frac{1}{4}$ inches of the mixed O. and H. disappear.

886. Three poles were made Pos. in the S.A. 1336 for five minutes. One was put into a fresh portion of S.A. 1336. Another was washed and put into *distilled water*, and the third washed, shaken and left in the *air*. After eight minutes each was put into a tube containing gas, *O. and H.* The air pole acted most rapidly; in four minutes its gases had nearly all disappeared, the tube becoming very warm. The acid pole acted next best as to rapidity and in about 7 minutes its gases had disappeared nearly. It was

observed in both these as the water rose that it rose against and covered the platina as if the latter were *quite dry*. It was also evident that as the warmth from first action increased, the effect went on more rapidly. Hence much difference may result in different expts. from accidental circumstance of water adhering more or less to the pole and requiring to be evaporated, so keeping down heat. The water pole was the most sluggish in commencing visible diminution; at last began to rise; after nine minutes rose rapidly, the *pole became red hot in the middle part* and the rest of the gas exploded.

887. Two thin foil poles and one thicker one were made Positive for 4 minutes in S.A. s.g. 1336. The two thin ones were removed and put at once into tubes; the thick one was wiped with filtering paper, the tube of gas lifted out of the water mouth downwards, the pole put up dry and then the whole put into a jar of water. The object being to dry the one pole and leave the other two wet. All three acted so nearly like that no difference could be traced to the drying.

888. Again the three were made Pos. in S.A. 1336 for 4 minutes. The thin ones were put as before into gas, but the thick one was washed in water, wiped with a cloth, washed and wiped again, after which it was also passed up through water into a tube of gas. The two first acted as before—not igniting but warming the tube, and the third or washed one acted but was slower than the former.

889. One vol. Nitrous gas was mixed with one vol. hydrogen—two vols. Nitrous oxide were mixed with one vol. hydrogen. These were put into tubes and four-minute Pos. poles from S.A. 1336 introduced. There was no sensible action after an hour even. The tubes were left for 36 hours.

890. A mixture of equal vols. of Chlorine and hydrogen was made, two tubes filled with it and four-minute Pos. poles from S.A. 1336 introduced. There was no apparent action at first. After some time a little action appeared visible. These were also left for 36 hours.

891. Two tubes with O. and H. and two four-minute poles (Pos.) from S.A. 1336 were prepared. One pole was put at once into its gas and instantly shewed action. The other pole was washed in water, heated red hot in the spirit lamp and then passed up through

water into its tube of gas. It did not act at first sensible—in three minutes it was sensible, and in nine minutes it had condensed nearly the whole of the gas.

892. Two thick poles and a thin one were made Pos. in S. A. 1336 for four minutes and put into tubes with O. and H., one of the thick ones having been washed and heated red hot in spirit lamp first. The two thick ones acted moderately and nearly alike. The thin one acted more quickly. I am inclined to attribute this to its more favourable condition as regards it becoming heated—the heat always favouring the action much.

893. Two poles were made Pos. for 4′ in Strong oil of Vitriol, then washed and put up into tubes of O. and H. They both acted well and quickly.

894. A pole, or as it should be now called, a plate, was merely heated by the spirit lamp and blow pipe (not having been connected with the battery) and put up into gas, O. and H. At first there was no action, but after a while condensation began and went on well at the last.

895. Another piece of platina plate was heated in the same way (not having been electrified), and put into mixed O. and H. It acted slowly, but at last all the gases nearly disappeared.

896. Two poles were made Pos. for 4 minutes in *diluted acetic acid*, and then put into mixed O. and H. They both acted very well, condensing the gases.

897. Two poles were made Pos. for 3 minutes in Mur. acid strong and pure. Much chlorine was evolved during the time. These poles in mixed O. and H. acted but slowly—decidedly less effectually than those from Acetic or Sulphuric acid. The disappearance of gases at last almost complete.

898. Two poles for 4 minutes in pure diluted Nitric acid. These acted pretty well on the mixed gases—better than the last—about as those from Acetic acid.

899. A piece of platina foil about 1 inch by $2\frac{3}{4}$ and which had not been used in any of these experiments, was curved so as to enter a tube and then put up into one containing O. and H., and left there at 8 o'clk. for the night.

900. This morning at 9 o'clk. the platina left last night in the O. and H. was examined. *No action or condensation had taken* place. It was now withdrawn from the gas through the water, heated by the Spirit lamp and blow pipe, and reintroduced into the same gas. In the course of a few minutes, condensation of the gas could be observed and in 45 minutes above $\frac{4}{5}$ of the gas or $1\frac{1}{4}$ inches (cubic) nearly, had disappeared. Hence heat alone can bring platina into the acting state.

901. The tubes with Chlorine and Hydrogen examined–about half of gas in each gone; what remained was nearly pure hydrogen, and chlorine was found dissolved in the water. It had also probably acted on the platina. Hence no combining power over chlorine and hydrogen, but this most probably due to *alteration of the surface of the platina* by the chlorine.

902. Now made some expts., not using the voltaic battery, but preparing the surface of the platina in other ways.

903. Cleaned a platina pole, i.e. one of the pieces of plate and wire before described, with a cork and some ashes from the fire place, washed it and put it up into mixed O. and H. It acted slowly at first, then well, and in less than an hour nearly all the gases combined.

904. Cleaned another platina pole with water and ordinary glass paper or fine sand paper. This in mixed O. and H. acted also but much slower than the last. In 2 or 3 hours the gases had nearly disappeared. The surface was scratched; that I think probably favourable, but the glue of the sandpaper perhaps deleterious.

905. A pole was boiled for a minute in dilute Nitric acid and then put into mixed O. and H. It acted very well, sooner than either of the former.

906. A pole was heated for about a minute in strong oil of vitriol, and then washed and put into mixed O. and H. It acted very well, as well as if it had been rendered Pos. by the battery.

907. A pole was boiled in a solution of caustic potassa and then washed and put into mixed O. and H. It did not seem to act at first–after a while acted sensibly–in 2 hours, $\frac{4}{5}$ of the gases had disappeared–the metal acting very well.

908. A pole was dipped in S. A., taken out and heated in the spirit lamp to drive off all the acid. This in mixed O. and H. acted very

slowly indeed, as if matter left by S. A. had soiled or injured the surface. After some hours only small diminution.

909. A pole boiled for a minute in strong Nitric acid—washed and put into mixed O. and H. acted very well indeed; gases soon gone.

910. A pole cleaned with whitening, water and a cork (after being burnished smooth by a glass stopper on glass) when put into mixed O. and H. acted slowly only, but in 2 or 3 hours gases had disappeared.

911. The tubes with Nitrous gas and Nitrous oxide examined. In the tube containing Nitrous gas and hydrogen, the diminution was very sensible, but very small; about $\frac{1}{8}$ of the gases had disappeared. This mixture when lighted burnt very slowly down tube.

912. The mixture of Nitrous oxide and hydrogen had diminished more than the last; about $\frac{1}{4}$ had disappeared; still action has been very slow. This mixture by light exploded.

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913. Yesterday a piece of amalgamated Zinc and a platina spatula were put into dilute Sul. Acid and left all night for the electric action to proceed. This morning, the platina was dull in most places but in some parts amalgamated. There had been precipitation in fact upon it. It was put up into a tube of O. and H. and left there for $2\frac{1}{2}$ hours, but no action ensued. It was then taken out and heated red hot, then cleaned by a cork, water and emery and returned into the gas. It instantly began to act and condense the gas, yet it had been a negative pole all night. It acted only slowly.

914. On moistening some of the poles to observe how the water disposed of itself, it was found sometimes to remain moistening the whole of the surface and sometimes to run off parts and gather together in drops.

915. One of the platina plates was burnished with agate, then rubbed with emery, a cork and water and put into tube of O. and H.; it acted slowly but decidedly. In about $1\frac{3}{4}$ hours about $\frac{2}{3}$ of the gas had disappeared.

916. A platina plate was burnished, then heated by spirit lamp and blow pipe and put into mixed O. and H.; it did not sensibly

act in $1\frac{1}{2}$ hours. A second was cleaned with emery, a cork and dilute Sul. acid, well washed and then heated by blow pipe and spirit lamp as the last; this was put into mixed O. and H. and did not act sensibly in $1\frac{1}{2}$ hours. This piece of platina, after being heated, looked tarnished; and as the spirit lamp had had salt upon the wick on a former day, a fresh lamp was trimmed with fresh spirit and a plat. plate previously cleaned by emery, sul. acid and a cork heated by it. This, put into O. and H., produced no effect in half an hour.

917. The three plates were now removed from the tubes and rubbed with emery, dilute S.A. and a cork so as to clean them. Being then returned each to its tube and gas, they all began instantly to act, and in about two hours had condensed above $\frac{2}{3}$ of the gas.

918. A plate platina cleaned on glass with emery, dilute Sul. acid and a cork—put into mixed O. and H.—soon acted sensibly—in 37 minutes $\frac{11}{12}$ of the gas gone—good.

919. Two poles made Pos. in S.A. 1336 for 4 minutes. One gave off much smaller bubbles than the other—being put into mixed O. and H., the first acted quicker than the second but both acted very well.

920. Two poles were burnished—cleaned with emery, dilute S.A. and a cork and then made Pos. in S.A. 1336 for 3 minutes by a weak and almost exhausted battery. They gave very small bubbles by battery—being put into mixed O. and H. they acted immediately, and in periods of 9 and 11 minutes nearly all the gases had disappeared.

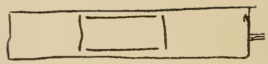
921. These two poles were then taken out of the tubes, washed and heated, one in a quite [? quiet] alcohol flame, the other by the same flame urged by blow pipe. They were then put into mixed O. and H. After an hour they shewed no action—in consequence they were removed and heated to fair redness and returned into gas again—still there was no action.

922. Three poles were made Pos. in S.A. 1366 for 5 minutes by the weak battery. One was then rinced, wiped and put up into mixed O. and H.; it acted very well. The second was dipped in Soapy water, washed and dried; being put into gas it acted

very well. The third was rinced, wiped very dry, passed over the tongue, again wiped well and put into gas; it also acted, and in half an hour nearly all three of them were up at the top.

923. The large curved platina plate which, when heated yesterday, succeeded in condensing the mixed gas, was to-day heated just in the same manner by the lamp and blow pipe and put up into mixed gases, but it did not shew the slightest effect.

924. A pole had a part in the middle cut round, thus—so that the middle portion adhered to the others only at the four corners. This was done to prevent the conduction away of heat and facilitate ignition. This and a second or ordinary plate were both made Pos. in S. A. 1336 for $3\frac{1}{2}$ minutes. The first was put into gas and acted very well but did not ignite. The second was put into distilled water and left there for 15 minutes, after which it was put into O. and H. mixed. In less than 2 minutes it began to rise and very soon rose rapidly, the *platina became red hot* and *the gas exploded*, blowing the tube out of the water.



925. I put the same pole immediately after into fresh O. and H.; it acted but very slowly upon it.

926. Now had a *new charge for the battery*.

927. Made three poles NEGATIVE for 4 minutes in S. A. 1336, and put each into a mixture of O. and H. They soon acted sensibly though not so strong as Pos. pole. In about 25 minutes, $\frac{4}{5}$ of gas in each had disappeared.

928. Hence Neg. poles will do it if clean.

929. Two poles made pos. in S. A. 1336 for four minutes to shew Mr. Daniell ignition, etc. etc. One put into tube of O. and H. shew absorption well; the other into distilled water for 10 minutes, then in mixed O. and H.; it soon began to rise, then rose rapidly and at last platina ignited but without exploding the gas, only making it disappear instantly. Hence pretty sure of this effect.

930. Six poles made Pos. in S. A. 1336 for 5 minute each at two separate times. These then left, two in similar acid, two in sol. potash and two in distilled water, to be examined as to their action on Monday.

931. This morning experimented with the six poles put away on Saturday night. It is now forty hours since they were connected with the battery.

932. The two retained in water were put into mixed O. and H. They acted, but very slowly and after some hours had caused but little condensation. Then took the worst of them—shook it, dipped it into warm oil of vitriol for one minute, then put it into water for a minute or two and then returned it to the same portion of mixed gases. It now acted very well, soon overtook the other and in a short time had caused far greater condensation than it.

933. Of the two retained in acid S. A. s.g. 1336, one was put at once into mixed O. and H., being only briefly rinced in water. It acted at once and well, and in 17 minutes, $\frac{3}{4}$ of the gas gone; action then diminished and the tube was taken off. The other was put into water for 10 minutes and then into mixed gases—it also acted very well, and gave very nearly the same result as the first.

934. Hence they keep pretty well in acid—but now the action of water in cleansing the surface not evident, in fact may consider that the weak acid do it—for when pole comes out of S. A. 1336 after being positive, the S. A. next to its surface is probably very highly concentrated, and this the weak acid could remove, almost as well as water.

935. Of the two poles retained for 40 hours in Alkali, one was simply rinced and put into mixed gases, the other was put for ten minutes into water and then into gases. They both acted sensibly immediately and finally acted very well. One of them rose so rapidly that I added gas twice to the tube, in hopes the plate would ignite, but it did not do so, the action gradually falling off.

936. None of these Six were as active as they would have been if after separation from the battery they had been put into water for 10 or 15 minutes, and then used. But the acid and alkali have kept them pretty well.

937. It appears also that in acting on the gas they lose power, but this probably from their accumulating on their surfaces the impurities of the O. and H. The oxygen was from Ox. manganese and the hydrogen from Zinc and S. A. Probably, Chlorate of potash oxygen and Hydrogen from Steam and pure Iron by heat, would be better.

938. Three poles which have not been used since Saturday were heated in Strong Sul. Acid for 10 minutes, then put into water for 10 minutes and then into tubes containing O. and H. They all acted immediately and well—as well as the best of the above six.

939. Whilst heated and in the S. A., the surfaces acquired such state as to cause much friction when the pieces were rubbed against each other. This no doubt because of their perfectly clean state, and helps to shew that effect is due to that clean state of surfaces which acid and battery induce.

940. The three above (938) were taken out of their tubes, put into distilled water for 10 minutes and then heated by a lamp trimmed with fresh cotton and spirit to avoid all impurity. The first was heated in quiet flame to fair redness, then put into mixed O. and H.—it acted slowly, about I think as it would have done if it had been put into a second portion of gas without any preparation.

941. A second was heated more highly by alcohol flame and blow pipe and put into mixed O. and H.—scarcely acted sensibly. After a while it was taken out of the gas—put in warm oil of vitriol for a minute—washed—and returned to the gas—it then acted immediately and well.

942. The third was still more powerfully heated than the second and then put into mixed O. and H. It acted better than the second and in $1\frac{1}{2}$ hour gas nearly all gone. It was then taken out—wiped clean with a cloth—heated red hot by lamp and blow pipe—put into fresh mixture of O. and H. and left. It acted slowly but fairly, and in about 33 minutes, half the second portion of gas gone.

943. Heat is evidently uncertain in its action, most probably because of what it finds on the surface of the plates, which it makes cleave by stronger force to them and because of what the fuel and air takes to it and deposits on it. It often happens that when heating the platina by the cleanest flame, the surface becomes cloudy and dulled, shewing deposition of something extraneous upon it.

944. Two poles were boiled in solution of pure caustic potassa (I found afterwards there was lead in this potassa)—then put into water for a few minutes—then in mixed O. and H. Neither acted sensibly in one hour. Then took one out of gas, put it into hot

Sul. Acid, strong, for half a minute, washed it and returned it to its own gas. It acted immediately though slowly and in 20 minutes $\frac{2}{3}$ of the gas was gone. The other, which had shewn no action in $1\frac{1}{2}$ hours, was taken out, was cleaned with a solution of potash, emery and a cork, put into water for 4 or 5 minutes and then returned to its own gas. It then acted sensibly although slowly, so that cleaning the surface gave power.

945. In order to obtain a notion of the adhesion of the acid or alkali to the platina, a pole was put into hot oil of vitriol, immediately taken out, washed, wiped—washed and wiped again, and then put into distilled water for 10 minutes. At the end of that time the water gave traces of sul. acid to Baryta.

946. In order to try if soiling surface would leave permanent harm, a pole was well dried—passed through the mouth to cover it with saliva—washed and wiped—washed and wiped again—then heated red hot by blow pipe and lamp and put into mixed O. and H. It acted slowly but directly, and finally very fairly. Hence heat does not destroy action *of necessity*, etc. etc.

947. A pole was oiled—the oil burnt off—the pole soaked in water a few minutes, wiped, washed, wiped, heated dull red and put into O. and H. As there was no sensible action in five minutes, it was taken out, washed again, heated much hotter and returned to the same gas. It then acted, but very slowly.

948. Must remember that platina can combine with carbon by heat, and that probably the surface thus aftd. in these modes of igniting.

949. The pole just referred to, being after a while cleaned with a cork, emery and dilute S. A., then acted at once, though not very fast.

950. Two poles were cleaned by Emery and dilute S.A., the cleaning friction being given by platina and not as heretofore by a cork. These being put into mixed O. and H. acted very fairly. In about 20 minute from $\frac{1}{2}$ to $\frac{2}{3}$ of the gases had condensed.

951. Two poles were boiled a few seconds in strong N.A., then put into water for 6 minutes and afterwards into mixed O. and H. They acted directly, and in five or six minutes nearly all the gas gone.

952. One of these poles was taken out—heated red in a clear

coal fire, not touching the cinders, and put into fresh mixture of O. and H. It now acted very slowly.

953. A pole was boiled in strong N. A. for 4 minutes, put into water for 2 minutes, and then into mixed O. and H. It acted at once and well, but did not inflame gases. In 9 minutes, $\frac{2}{3}$ of gas gone.

954. Two poles which, having been rubbed with platina and emery were very rough and dull on surface, were put into S. A. strong for 20 minutes, then into water for 3 minutes, and then into gas. They acted very fairly, but not powerfully. They had not been in water long enough considering their roughened state.

955. A pole was cleaned and rubbed by a piece of charcoal and water, then washed, put into mixed gases and acted slowly directly—it acted very well afterwards. The top of tube became warm, and in 20 minutes $\frac{3}{4}$ of the gas was gone.

956. A pole was cleaned with oxide manganese (per), a cork and water, then put into gas, O. and H. It acted at once slowly and in 25 minutes $\frac{5}{6}$ of the gas was gone.

957. A burnished plate pole was put into hot strong S. A. for about half a minute, then into distilled water, shaken about and wiped with a clean cloth—again into distilled water (fresh), shaken about and wiped—again into fresh distilled water, where it was moved about for 5 or 10 seconds, and then into fresh distilled water where it was left for five minutes. The third and fourth portions of water were then tested by Mur. Baryta. No sensible trace of S. A. appeared in the third, but the fourth gave very sensible indications. Very considerable considering the circumstances. Proof of strong adhesion of or retention of the S. A. and therefore of other bodies by the platina.

958. This effect very probably a general one.

959. All the previous expts. of to-day have been made without battery.

960. Four poles were made, two Pos. and two Neg., in S. A. s.g. 1336 for seven minutes. One Pos. pole was rinced in water and then put into mixed O. and H. It acted very well indeed. The other Pos. pole was put into water for 10 minutes and then into gas, O. and H. It instantly acted—the gas rose very fast and the plate became so hot that the water boiled as it covered it, but it

did not explode. One of the neg. poles was rinced in water and then put into O. and H.—it acted very distinctly, but slowly as compared to the P. poles. The other Neg. Pole was put into water for 10 minutes and then into mixed O. and H.; it acted much faster than former N. Pole, perhaps twice as fast, but yet slow as compared to the P. poles.

961. Two poles were made, one P. one N., in strong sol. of sul. soda for 5 minutes; then put into separate portions of water for 10 minutes and then into O. and H. The P. pole soon acted, the water ran up fast, the gas almost exploded. The N. Pole also acted and in 15 minutes had condensed ½ the gas or more. There was no lead in this saline solution.

962. A Pos. and a Neg. pole were made for five minutes in sol. of Potassa (it contains a little lead), then in water for 10 minutes, then in O. and H. They seemed to exert no action at all. After 40 minutes, during which there was no action, they were both cleaned by emery, water and a cork, returned to the gases and then acted slowly. The Neg. pole was then cleaned by emery, dilute S. A. and a cork, after which it acted better. Being then dipped in hot strong S. A., put into water and then into the gas, it acted better still.

963. The four poles of (960) which had been in seven minutes, etc. etc. were now made poles in S. A. s.g. 1336, for 5 minutes. Those which were *pos. being now neg.* and those which were *Neg. being now Pos.* These were put into water for 30 minutes and then into O. and H. The two pos. poles quickly acted: one produced ignition of itself, but did not explode gas; the other rose very rapidly—the water evaporated fast from off it—it became so hot as to make the rising water boil, but it did not visibly ignite, though it was close upon it. The action of both was over in a few minutes. The Neg. poles both acted, and fairly, though far more slowly than the others; in 30 minutes, $\frac{4}{5}$ of the gas of each had disappeared.

964. Two poles were made P. and N. in a solution of Carbonate of Potassa for 5 minutes—then put into water for 5 minutes and then into mixed O. and H. There was no sensible action in half an hour. The N. pole was then dipped in Oil Vitriol—then in water and returned to the gas. Now acted very well—in seven

minutes the gas was up at the top, the tube becoming warm. Did the same with the P. Pole—action—but not so good as last.

965. Now dipped a pole in sol. potash, dried it by spirit lamp and almost fuzed the remaining alkali upon it. Then put it into water for 4 or 5 minutes—next into strong Oil Vitriol hot for a minute or two—then into water for ten minutes, and then into O. and H. Instant excellent action, the gas rose quickly, the platina became red hot, and thus Dobereiner effect was produced *without action of battery* on the platina. *Good.*

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966. Query the temperature at which bodies combine without contact of solid matter. Put jet of gas in place of blow pipe current—then put wire or some incombustible substance there and see if it inflames it. Probably gas require a very high temperature to inflame without solid matter.

967. In all cases of common inflammation solid matter is present —great use of carbon in fuel in consequence in this point of view. Carbon probably burn sooner than either hydrogen or carb. hydrogen.

968. General views of this kind.

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969. Refer phenomena to attraction of Particles, yet not sufficient to cause chemical combination. This probably made a general principle. As cases make [? may] take Aggregation of similar particles as in Pottery. Glass attracts water, resin does not—a similar case almost of chem. selection. Then adhesion of air as to needles when floating on water; to glass in Bellani's thermometers and barometers, etc. Wetting of different substances, some by water, some by oil, etc. etc., yet not soluble. Resin and metals, not wet well—glass, silica, etc. very well. Nuclei: their action especially in vapours—will glass and metal differ in condensing camphor—or things in solution when crystallizing, will they adhere differently.

970. Quick loss of power is a proof of attraction of other and interfering matters. Then especially the adhesion of Sulc. Acid, etc. etc.

971. When metals made poles of battery the acid and alkali in

immediate contact most probably very strong and in very close contact; circumstances are very favourable.

972. Observe what is the appearance as to wetting of 4′ plat. poles—when directly taken out of the acid—when washed for 15′; and also of common platina. Does it wet equally in all cases? How does it wet after heat? If it wets least after washing, it will shew the removal of the acid by washing—it will also shew a quicker contact of gas in such cases and therefore ought to be quicker effect.

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973. Ritchie says that current in passing a decomposing solution is as magnetic as in a wire. If so, very strange. Would result evidently that it did not pass in consequence of decomposition or by any process of that kind, for no reaction on it more than when it passes without, as in wire.

974. If it should turn out that electricity can decompose a salt in solution as well as the water, and without diminution of action on the latter, then again shew independance of conduction on decomposition, but dependance of latter on the former—this with its unaltered magnetic action very curious.

975. Perhaps conduction and little decomposition of Sulet. of Silver also prove the same thing.

976. Will be very strange if Electricity can decompose two bodies at once to an equal equivalent amount, as it can if they were acted on separately. It will shew relations altogether new as compared to other powers of matter in their usual or natural condition.

977. In the transfer of Sulphuric acid (in expts. on dilute S.A.) must consider the effect of the connecting asbestus and how far it might help. If my view of Porret's phenomena right, it might help much.

978. In investigating equivalent decomposition and constant decomposition by the voltaic battery, after shewing water constant, then try whether it can any way be varied. That done, take other substances in a simple condition, as fuzed chlorides, etc. etc., to compare with water; and then take cases in which two substances, as water and a salt, give way at once, and see if decomposing action

on both is not equal, and therefore double what it would be on either alone.

979. As the nature of the pole seems to make no difference in the sum of action, may tell the acid taken by using a positive zinc pole in a neutral solution and ascertaining how much alkali or earth or base is set free; using a standard sulphuric acid to measure this evolved base and therefore represent the acid combined.

980. If a zinc Pos. pole is used in Sul. Soda, will an equivalent of hydrogen and another of alkali appear at the negative pole. Probably find good use of diaphragm in these cases, to keep acid and alkali apart.

981. For separation of base and hydrogen at once may use Muriate, Nitrate or perhaps Acetate of baryta, lime, strontia, magnesia, etc. etc. with a *Pos.* pole that will unite to acid.

982. Must ascertain if there is any way of altering the quantity of water decomposed, as by combining its oxygen or hydrogen at the poles or putting other decomposing bodies with it.

983. As to *smell of Electricity.* Does it occur at the Pos. pole only? in acid? May it be due to power of *Pos.* pole to combine bodies? Can it occur at both Pos. and Neg. surface of machine electricity? Combining power at P. surface may be very strong because very concentrated as to surface (i.e. to its extent). Cavendish acid may perhaps be formed only at P. Pole and be a consequence of this peculiar power? Should then happen with Brushes equally with sparks. Try smell of Electricity from Wooden points—in two portions of air separated by water or something not giving or causing sparks. Pass smelling air through a thick cloth moistened with solution of potassa, etc. etc.

984. Bonijol's effect of decomposition perhaps connected with these phenomena. Wollaston's also.

985. Conductibility mutually conferred on each other by Sulphurous acid and water. De la Rive, Quar. Jour., xxvii. 407. Chlo. Arsenic same (373 exp.).

986. On necessary intensity for decomposition, Do. 1831, p. 377.

987. Ampère and Becquerel on Electricity produced by combination and nature of resulting current. Ann. de Chimie, xxvii, p. 29. This reference is for consideration of Source of Voltaic

electricity and electrical nature of chem. combination. See Also by same, the end of a memoire presented to the Acad. on 3 Decr. 1823.

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988. Crumpled up the end of a thin wire—dipped the crumpled part into strong sol. potassa (it contained a little lead)—heated it in lamp until alkali fuzed—soaked it in water for 10 minutes—dipped it in nearly boiling Sul. Acid (strong) for 3 minutes—put it into water for 10 minutes—shook it well and then held it over a small jet of hydrogen; but it did not shew any signs of heating. When warmed in lamp and then put into jet it cooled down to common temperature, unless it were of considerable temperature (not visibly red), and then its temperature rose to redness and it soon inflamed the hydrogen. Do not expect to render wire effective with a common jet of hydrogen in air. No doubt of it in mixed O. and H.

989. Prepared oxygen from chlorate of potassa, and hydrogen by passing steam of water over heated iron; the latter has a strong smell and is probably far from pure.

990. A *Gold* plate was made pos. for 5 minutes in Sul. Acid, s.g. 1336, by a weak battery of 40 Pr. plates—put into water for 10 minutes and then into mixed O. and H., the hydrogen being that from iron and steam. There was no action here in 2½ hours. Then took it out, made it Pos. for 9 minutes in the S. A. 1336 by the same half exhausted battery, and put it into O. and H. (the hydrogen being from zinc, and found effective with platina). In 3 hours and quarter still no action. Then took it out and made it Pos. in S. A. 1336 by means of a newly charged and active battery of 40 pr. plates for 5 minutes—left it in water for 10 minutes, and put it into fresh good O. and H. at 7 o'clk. P.M. to be left all night.

991. On the 8th at 7 o'clk. P.M., i.e. after 24 hours. The action of this pole had been very distinct; gas had disappeared. Still the action had been but slow.

992. A *Palladium* plate made Pos. for 5 minutes in S. A. 1336 by the weak battery. It evolved oxygen but it was also acted upon and a solution of Palladium formed—it was put into water for 10 minutes—then into O. and H. (the hydrogen that from iron and

steam) and left for 2 hours, during which there was no appearance of action. It was then taken out, made Pos. for 9 minutes in the S.A. 1336 by same exhausted battery and put into *good* O. and H. (H. from Zinc and S.A). It now acted immediately and went on very well though not like platina.

993. A Platina plate was made Pos. for 3 minutes in S. A. 1336 by the same half exhausted voltaic battery, and put into water for 10 minutes, and then into O. and H. (the H. from Iron and Steam). There was *no action* in $6\frac{1}{2}$ hours. It was then taken out and made Pos. for 5 minutes by newly charged active battery of 40 Pr. plates in S. A. 1336–put into water for 10 minutes and returned to the *same* O. and H. at $\frac{1}{4}$ p. 7 o'clk. P.M. to be left for 24 hours. Being examined after 24 hours, i.e. at $\frac{1}{4}$ p. 7 o'clk. P.M. on the 8th instant, there was so small a diminution that the occurrence of any real action was doubtful.

994. A similar plat. plate made Pos. for 3′ in S.A. 1336 by same exhausted battery–put into water for 10′ and then into O. and H. (the hydrogen from zinc and S. A.)–acted very well and the platina *ignited.*

995. Hence Iron hydrogen bad, probably from compound of carbon in it with hydrogen or some other matter, and is remarkable that Oleft. gas apparently interfered with action of Plat. Zinc hydrogen good, but probably O. and H. by battery poles better still. Must try silver, etc. etc. etc., by such O. and H.

996. See if little Oleft. or Carb. oxide will prevent action of good Oxy. and H.

997. Two *silver* plates inches long wide and of inch in thickness were made Pos. for 5 minutes in S. A. 1336, by old battery. They were acted upon, silver was dissolved and the surface became dull. They were put into distilled water for 20 minutes and then into good O. and H. (Zinc hydrogen). They shewed no sensible action and were left at 8′ to 2 o'clk. After 24 hours, still no action. Being taken out, they seemed dirty; a finger passed over them moved a black powder.

998. Two Platina plates were made Pos. for 10 minutes by exhausted battery in a solution of *Tartaric acid* consisting of 1 vol. sat. solution and 2 vols. water. They were then put into water for 9 minutes and afterwards into good O. and H. Both acted very

well, but did not rise to ignition. In 20′ gases nearly all gone—tubes became warm.

999. Two platina plates were made Pos. for 7 minutes by old weak battery in diluted *Acetic acid*, and then put into water for 10 minutes—being put into good O. and H., they acted very well. One rose to ignition and explosion, the other very nearly so.

1000. Two platina plates made Pos. by the exhausted battery for 7 minutes in diluted solution of *Oxalic acid*—then put into water for 15 minutes and finally into good O. and H. They both acted very fairly but not as those from Acetic acid.

1001. Two platina plates, first heated, then made Pos. for 8 minutes in a dilute solution of Acetate of Potassa by the exhausted battery—put into water for 16 minutes—then into good O. and H. They acted but slowly, and in 23 minutes had not condensed above $\frac{1}{3}$ of the gases—but this was due to poor state of battery.

1002. Two plat. poles into sol. *sul. Zinc*. Here the P. pole became yellow and finally covered with a reddish body which could be rubbed off in scales; which was not dissipated by heat, though something altered; which dissolved in Strong M. A. forming a dark red solution, becoming colourless by heat or dilution, etc. etc. It does not look like per oxide of lead, and must be examined—it looks rather like a NEW BODY.

1003. Two plat. plates in sol. sul. copper for 10 minutes, being rendered Pos. by the old exhausted battery—they were then put into water for 8 minutes, and afterwards into good O. and H. There was no sensible action in half an hour. The plates were then withdrawn from the gases, made pos. in the *same solution* of *sul. copper* for 5 minutes by a *freshly charged battery*, put into water for 10 minutes and returned to the same portions of gases in which they were before. They now both acted very fairly. The old battery had been too weak.

1004. A Platina pole rendered Pos. for 5 minutes by fresh battery in S. A. 1336, then in water for 10 minutes, then into a mixture of 2 vols. Carb. oxide and 1 vol. oxygen. This was done at 10′ to 7 o'clk. P.M. and left for 24 hours (Short Jar). At 7 o'clk. P.M. on the 8th Novr., there had been no sensible action.

1005. A Plat. pole made pos. for 5 minutes by fresh battery in S. A. 1336—in water for 10′, then in mixture of 1 vol. Oleft. gas

and 3 vols. Oxygen—at 10′ to 7 o'clk. P.M. to be left 24 hours (taller Jar). At 7 o'clk. on the 8th, i.e. after 24 hours, no sensible action had occurred.

1006. A thin Plat. pole made Pos. for 5′ by good battery in a strong solution of Carbonate of Soda—then in water for 10 minutes, and then in good O. and H., at $\frac{1}{2}$ p. 7 o'clk. P.M. for 24 hours. At $\frac{1}{2}$ p. 7 o'clk. on the 8th, i.e. after 24 hours, but very little action had taken place; about $\frac{1}{8}$ of the gas had disappeared. There had been action.

1007. Two plat. poles made Pos. for 5 minutes by good battery in S.A. 1336, then put into water for 10 minutes, after which they were quickly shaken and put into a little bottle containing only air, and the mouth closed (not air tight) by a cork. The bottle was then inverted with its mouth in water and put by at 20′ to 8 o'clk. P.M. to be left for 40 or 50 hours, i.e. until Saturday or Monday next.

1008. On Monday the 11th, at 14′ p. 9 o'clk., i.e. after 84$\frac{1}{2}$ hours, one of these was put into good O. and H. It acted at once and exploded the gas at 19′ p. 9 o'clk., i.e. in 5′ from its introduction.

1009. The other pole was put into good O. and H. at 16′ to 10 o'clk. A.M.; on Friday the 15th, i.e. after 7$\frac{1}{2}$ days, or rather 182 hours, it acted very well, though not rapidly. All the gas was gone in about two hours.

1010. A Plat. pole made Pos. for 5 minutes by good battery in Sul. Acid 1336, then put into a small portion of the same acid and set aside at 15 minutes to 8 o'clk. P.M. to be left 6 or 8 days, i.e. until Thursday or Friday next. It was put into O. and H. on Friday the 15th at 10′ to 10 o'clk., having been in the acid 7$\frac{1}{2}$ days or rather 182 hours. It acted directly and in 27′ the gas was all gone.

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1011. A little spongy platina reduced as a ball on the end of a wire and suspended in a glass tube, so that a spirit lamp put beneath should touch it with its flame and keep it red hot. Being heated for 3 hours, then cooled and afterwards held in a jet of hydrogen, it ignited freely. After it had been heated six hours and subject to the gases from the spirit lamp, still it acted very well. Being

heated an hour longer, i.e. 7 hours altogether, still it acted well as spongy platina. Hence heat in this way does spongy platina little if any harm.
1012. Crumpled up the end of a small *wire*, heated it in Potassa, then in water, then in hot oil vitriol; then in water, shaken and held in jet of hydrogen, but could not cause ignition. Could perceive no hopes of raising the power of a *wire* so as to effect this action.
1013. Made a *silver plate* Pos. in dilute acetic acid. It was powerfully acted upon but became much discoloured. I thought there was no hope of finding it active on O. and H. and did not pursue the experiment.
1014. Cleaned two plates of *silver* with cork, water and fine emery, washed them and put them into O. and H. at 18′ p. 3 o'clk. On Saturday the 9th, after 26 hours, no action. On Monday the 11th at 9 A.M., after 66 hours, NO *sensible action*. On Tuesday the 12th at 9 P.M., no action.
1015. Made a Plat. plate Pos. in sol. of Nitrate of Lead—per oxide of lead formed on it so as to stop the expt.
1016. The tints are very good and from their order would serve well to indicate the intensity of electric currents in different parts of a solution under investigation. *GOOD*.
1017. Boiled a plat. plate in strong sol. of Ammonia—put into water for a few minutes, then into O. and H. at 12′ to 4 o'clk. No action in 3 hours. Then taken out—dipped in hot Sulphuric acid—put into water for 10 minutes and returned to the same gas. It soon acted, but only slowly. In 27 minutes about $\frac{1}{5}$ of the gas gone.
1018. A Platina Plate made Pos. in S.A. 1336 for 8 minutes—then put into water for 10 minutes. A mixture of 3 vols. O. and H. with 1 *vol. Oleft. mixture* (i.e. of 8 hy. + 4 ox. with 1 Oleft. + 3 ox. vols.) was prepared, and the plate put in at 11′ p. 7 P.M. At $\frac{1}{4}$ to 11 o'clk. next day, i.e. after $15\frac{1}{2}$ hours, there had been no action. This was because of the *Oleft. gas* present, for on taking out the plate and putting it into O. and H., it acted at once and in 17 minutes the gas was gone.
1019. A Platina plate made Pos. in S.A. 1336 for 8 minutes, then put into water for 10′. Then into the following mixture of gas

at 11′ past 7 o'clk. P.M.—namely, 3 vols. O. and H. with 1 vol. Carb. ox. mixture (i.e. 6 hy. + 3 ox. with 2 c. ox. + 1 ox.). There was no sensible action in $15\frac{1}{2}$ hours. On taking out the pole and putting it into good O. and H. it acted also and very fairly. All the gas was gone in 40 minutes.

1020. Hence both these mixtures retard the action of the platina on O. and H.

1021. An unused platina plate was boiled in a strong solution of *Tartaric acid* for 5 minutes—then put into water for 8′, and then into O. and H. at 18′ p. 7 o'clk. There was no action next day at 15′ to 11 o'clk., i.e. after $15\frac{1}{2}$ hours.

1022. An unused platina plate was boiled in strong Pharmacopæia Acetic Acid for 5′, then put into water for 10 minutes and then into O. and H. It acted at once and very well.

1023. A Platina plate wetted in Potassa and heated red in spirit lamp flame—it became stained in one part. It was put in water to remove the alkali—then boiled in strong Acetic acid for 5′—put into water for 10′—and then into O. and H. It acted at once, notwithstanding the stained state, and the gas was all gone in 37 minutes.

1024. A platina plate wetted in potash and heated red in spirit lamp flame; it became stained in one part—it was then soaked in water to remove the alkali—boiled for 5′ in solution of Tartaric acid—put into water for 5′ and then into O. and H. It acted directly and in 15′ exploded.

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1025. A platina plate made Pos. for 20 minutes in S. A. 1336 by a weak voltaic battery—put into water for 10′ and then into a gaseous mixture of 11 vols. O. and H. with 1 vol. *Oleft mixture* (i.e. of 29·3 H. + 14·6 Ox. with 1 oleft. + 3 ox.) containing only $\frac{1}{48}$ Oleft gas. This was at 35′ p. 11 A.M. On the 11th at 9 o'clk. A.M., i.e. after $45\frac{1}{2}$ hours, there had been *no sensible action.* The plate was then taken out of that mixture and put into good O. and H. on which it immediately acted; the gas was all gone in 7′, and very nearly exploded.

1026. A Platina plate made Pos. for 20′ in S. A. 1336 by a weak battery, and put for 10′ into water. A mixture was made of 11 vols.

O. and H. with 1 vol. *carb. ox.* mixture (i.e. of 22 H. + 11 ox. with 2 c. ox. + 1 ox.) containing therefore $\frac{1}{18}$ carb. oxide. Into this the plate was put. It acted slowly at first—in 25′ it was still acting slowly, but at the end of 42′ it exploded.

1027. A plate made Pos. in S. A. 1336 by weak battery for 20′, then put into water for 10′. Filled a dry tube by a jet with O. and H. closed it with the finger, dry. Dried the plate with filtering paper and then put it into the tube quickly through the air (the tube being held mouth downwards) and then the tube put with its mouth into a dish of water. The action of this dry pole on dry gas was very good. In 3′ all the gas was gone, but the effect did not quite rise to explosion.

1028. A platina plate made Pos. in S. A. 1336 by weak battery for 20′ and then put into water for 15′. This was put into a tube with a little distilled water and set aside at 10′ p. 12 (Midday). After 96 hours, i.e. on 13th, it was put into O. and H. It acted slowly—in 9 hours the gas was about $\frac{3}{4}$ up. Before 24 hours over quite up.

1029. Equivalents of *Carbs. Pot. and soda* mingled, sprinkled over a platina plate and fuzed on it by lamp or fire. No action here on the platina—then put into water to wash off the carbonates—put into fresh water for 10′ and then into O. and H. at 35′ p. 1 o'clk. In 25′ they were acting slowly. In 2 hours about $\frac{1}{10}$ of the gas gone—but the action very slow. It might however have risen, but the pole was taken out, dipped into S. A. (hot) for a moment, then into water for 5′, and then into fresh gas: it now acted at once and in 15′ *exploded.*

1030. A platina plate fuzed with the *Carb. pot. and soda* on it—put into water—then into another water—into hot S. A. for 5′—into water for 9′—into O. and H. It acted directly and in 10′ almost exploded, the gas running up rapidly.

1031. A plat. plate heated and fuzed with Borax sprinkled on it—put into water—boiled in dilute S. A. until Borax dissolved off—put into water—put into O. and H.—acted slowly only and in $1\frac{1}{2}$ hours about $\frac{1}{5}$ of gas gone. Taken out—put into hot oil vitriol for a moment—then into water for 5′—into O. and H.—it acted at once and in 15′ gas all gone.

1032. A Plat. plate dipped in *Phosphoric acid* and heated in spirit lamp flame—then put into water for 10′ and then into O. and H.—

no apparent action in $1\frac{1}{2}$ hours. Taken out of gas and put into hot S. A. for a moment–then into water for 8′–and then returned it to the same gas: it acted at once–in 24′ about $\frac{2}{3}$ of the gas gone.

1033. A Platina plate heated in spirit lamp flame and rubbed over with a piece of *potassa fusa*–put into water to remove the alkali–into *hot Sul. Acid* for a moment–into water for 15′–into Ox. and H.–it acted slowly at first and exploded at the end of 12 minutes.

1034. A platina plate acted on in the same way by potassa fusa –heated in the fire until the plate much tarnished–put into water to remove alkali–into hot oil vitriol for 1′, which *did not remove* dark stains–into water for 15′–into O. and H. at 22′ p. 4 o'clk. It *caused explosion* in 6′.

1035. This pole or plate was put into a little water and set aside until monday, on which day at 21′ p. 9 A.M., i.e. after 41 hours, it was again put into O. and H. At first it acted but slowly–in 30′ gas about half gone–in 40′ nearly all gone, and it rose so fast at last that it was evident if there had been enough gas at last it would have exploded.

1036. A plate of *Palladium* was dipped into hot Sulphuric acid for one minute–into water for 10′–into O. and H.–it acted very fairly at once.

1037. A plate of *Gold* in hot Oil Vitriol for 5′–in water for 12′–then into gas, O. and H.–it acted very fairly at once.

1038. A plate of *copper* cleaned by emery and water–put into hot S. A. for a moment (it instantly tarnished)–into water for 11′–into O. and H. at 5 o'clk. P.M. On Monday the 11th at 9 o'clk. A.M., i.e. after 40 hours, *no action*.

1039. A plate of *copper* cleaned by emery, cork and water–put into water for a few moments to rince off cleanings and then into O. and H. at 5 o'clk. On Monday the 11th, at 9 o'clk A.M., i.e. after 40 hours, *no action*.

1040. Two *Plat. plates* made Pos. in S. A. 1336 for 20′ by an exhausted battery–put into water for 55′, and then put into the following mixtures of gases at 5′ to 6 P.M.

1041. 49 *Vols. O. and H.* + 1 *vol. Oleft. gas*–the action hardly sensible in 2 hours time–but on looking 24 hours after, the tube

was found blown to pieces. The retardation at first occasioned by the Oleft. gas very sensitive here.

1042. 99 *vols. O. and H.* + 1 *vol. Oleft. gas*. The action was sensible in 50′—went on slowly until the end of 85′—gas then rose rapidly and exploded. Hence, also effect of olefiant gas in retarding evident and proportionate.

1043. A Thin Plat. plate, heated in lamp and rubbed over with potassa fusa—then in water—then in hot S.A. for 4′—in water for 12′—then in mixture of 99 ox. and H. + 1 oleft gas. It acted sensibly at once—went on steadily—at the end of 50′ about $\frac{2}{5}$ of gas left—it then rose suddenly and all the gas disappeared at once, almost producing explosion.

1044. The effect with these proportions may vary when stronger battery used or other changes made—but general results very good and instructive.

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1045. Experimented with O. and H. mingled with different proportions of air and other gases. Thus six equal plat. plates were prepared by heating them in spirit lamp, rubbing potassa fusa over them but not continuing the heat until the platina was discoloured—putting the plates in water to wash off the alkali—dapping them between filtering paper to dry them—putting them into hot oil of vitriol for 5′—and then into one portion of distilled water to wash roughly and into another portion to remain 10′. These plates were put into the following mixtures—

AIR	i.	O. and H. with air = $\frac{1}{2}$	acts well in 20′	$\frac{1}{5}$ of whole bulk gone	In $2\frac{1}{2}$ hours nearly all in each gone = of the O. and H.
	ii.	,, ,, = $\frac{1}{4}$	Do. — 20′	$\frac{1}{6}$,, ,,	
	iii.	,, ,, = $\frac{1}{8}$	Do. — 20′	$\frac{1}{6}$,, ,,	
	iv.	,, ,, = $\frac{2}{3}$	Do. — 20′	$\frac{1}{9}$,, ,,	
OXYGEN	v.	,, with oxy. = $\frac{1}{2}$	Do. — 20′	$\frac{1}{15}$ of whole bulk gone	In $2\frac{1}{2}$ hours nearly all the O. and H. to form water gone.
	vi.	,, ,, = $\frac{1}{4}$	Do. — 20′	$\frac{1}{5}$,, ,,	

At first starting i, ii, iii were nearly alike—iv slower and v about as iv—vi twice as fast as v.

1046. Oxygen I think retards more than air, but that is doubtful, and much will depend upon the state of the plates—not certain all alike. It is remarkable to see $\frac{1}{2}$ or $\frac{1}{4}$ of air do so little—even $\frac{2}{3}$—not retard much, considering the dilution that must occur at place of condensation.

1047. Prepared five plates as described, by Pot. and S. A. They were in S. A. 5′—in water 13′. Were then put into O. and H. diluted with Hydrogen.

O. and H. with Hydrogen = $\frac{4}{5}$
" " " $\frac{2}{3}$
" " " $\frac{1}{2}$
" " " $\frac{1}{4}$
" " " $\frac{1}{8}$

All acted at once, but iv and v quickest—then iii, then ii, then i. In 1 hour 10′, nearly all the O. and H. which could form water gone. Action very good and quick. Hydrogen therefore retards very little, astonishingly little. In these and the other expts., $\frac{4}{5}$ means that $\frac{4}{5}$ of the whole bulk was diluting hydrogen, the rest O. and H. to form water. HYDROGEN

1048. Prepared five plates for gas diluted with Nitrogen. Were in hot S. A. 5′, in water 12′. Then in gas.

O. and H. with Nitrogen = $\frac{4}{5}$
" " " $\frac{2}{3}$
" " " $\frac{1}{2}$
" " " $\frac{1}{4}$
" " " $\frac{1}{8}$

They all acted at once, v, iv and iii together and best, ii and i less. In 1 h. 10′ the O. and H. not all gone, though not far from it. In $\frac{1}{2}$ hour more still, some O. and H. remaining. Nitrogen retards more than Hydrogen I think, especially at last, but must remember that it was prepared by Phosphorus from air and that the plates may have become a little soiled by Phos. acid. NITROGEN

1049. Prepared five other plates, etc. in S. A. 7′, in water 12′.

O. and H. with Nitrous Oxide $\frac{4}{5}$
" " " $\frac{2}{3}$
" " " $\frac{1}{2}$
" " " $\frac{1}{4}$
" " " $\frac{1}{8}$

They all act at once well in the order v, iv, iii, ii, i. In 46′, all up, all O. and H. gone. This gas retards I think decidedly less than nitrogen or air. I have an impression it is about as hydrogen. NITROUS OXIDE

1050. Prepared a plate platina—in S. A. for 5′—in water 12′. A tube of O. and H. prepared, a little piece of cork dipped in ether and ETHER

passed up into the gas. Then the plate in. It acted at first fairly, went on slowly. In 2 hours it was nearly at the top—and next morng., i.e. in 14 hours, quite up. Consequently the ether had taken no oxygen—the tube being open smelt of ether. Ether does not therefore retard much.

OIL GAS VAPOUR 1051. A plate prepared by Alkali, acid, etc. in S.A. 5′, in water 12′; a tube filled with O. and H. and then a piece of cork dipped in the volatile liquor from the condensed oil gas passed up. The plate was then introduced and acted but very slowly. In 2 hours only about 1 inch up. On Wednesday after 14 hours about ⅔rds up. On Thursday after 39 hours about $\frac{3}{5}$ up. On Friday after 60 hours about $\frac{5}{6}$ up. At this time the tube with its remaing. gas was removed and the plate put into good O. and H. The tube smelled strongly of the oil gas liquor. The plate in the fresh tube acted at once; in 12 hours it was and in 24 hours it was up at the top. The action therefore had not been destroyed but quelled.

SULPHURET OF CARBON 1052. A plate prepared by Alkali, acid, etc. in S. acid 5′, in water 12′. A tube of O. and H. had a cork dipped in sulphuret of carbon put up, and then the plate introduced. After 2 hours no sensible action. On Wednesday after 14 hours scarcely any action, only about ½ inch of gas in tube gone. On Thursday after 39 hours the same. On Friday after 61 hours up a very little more; scarcely any action. Now put the plate into a fresh tube of oxygen and hydrogen and examined the old tube. It smelt strongly of sulphuret of carbon. The plate in fresh gas acted very well and in 18′ had condensed the gas. Hence great retardation by vapour but no injury of plate.

1053. A plate prepared like the three last put into good O. and H. for comparison. Acted at once well, did not run up, but in 2 hours all gone. I think that using the water through which the sulphuret of carbon and other matters had passed may have caused the retardation by sending up a little vapour to the gas of this expt.

1054. Six plates prepared by Pot. and Acid. In S. A. 6′—in water 20′.

i. O. and H. with Carb. oxide = $\frac{4}{5}$
ii. " " " $\frac{2}{3}$
iii. " " " $\frac{1}{2}$
iv. " " " $\frac{1}{4}$
v. " " " $\frac{1}{8}$
vi. " " " 0

No sensible action in any in 3 hours time except the last; that acted at once—but slowly. All was gone in it in 1 hour. CARBONIC OXIDE

Then prepared five other plates. They were in hot S. A. for 7′, and in water for 12′. They were then put into these gases, and the plates taken out, put into good O. and H.; the latter immediately acted very well and soon condensed all the gas, but the former did not act at all. These new plates were left in until Thursday, but though 20 hours had passed no action had occurred. The plates were then taken out of the tubes i, ii and iii and put into good O. and H.; all acted at once, and in 17 minutes all the gases were condensed. The tubes iv and v were left till Friday, but though 42 hours had passed no condensation had occurred. The plates being then taken out and put into good O. and H. acted very well; the gas rose rapidly and in 10′ was up at the top, producing almost inflammation.

1055. Hence carbonic oxide retards the action in an extraordinary way, but yet without injuring the plate in the least. Even $\frac{1}{8}$ prevents action entirely.

1056. Five plates prepared by alkali, etc. In S. A. 8′, in water 18′.

O. and H. with Carb. acid = $\frac{4}{5}$
" " " $\frac{2}{3}$
" " " $\frac{1}{2}$
" " " $\frac{1}{4}$
" " " $\frac{1}{8}$

They acted very fairly at once, the last most rapidly. In $1\frac{1}{4}$ hours all nearly finished. It was very evident from the shortness of the time and notwithstanding the absorbability of carbonic acid by water, that this gas causes but little retardation to the action, only that which is mechanical. It is a singular contrast to Carbonic oxide. CARBONIC ACID

1057. I changed the acid in the tube which has been in use some time, for it was much weakened by spontaneous dilution, and I found much quicker effect with plates prepared with the new acid.

1058. Five plates prepared with Alkali, etc. In S. A. 5′, in water 15′.

OXYGEN O. and H. with Oxygen $=\frac{4}{5}$ | ” ” ” $\frac{2}{3}$ | ” ” ” $\frac{1}{2}$ | ” ” ” $\frac{1}{4}$ | ” ” ” $\frac{1}{8}$ } Action very fair but not so strong as with Nitrous oxide or hydrogen or Nitrogen. In 1 h. 18′ the O. and H. to form water nearly gone. Must compare different gases at once, since different preparation of plates may interfere.

PHOSPHURETTED HYDROGEN 1059. A platina plate prepared with alkali, etc. In S. A. 5′, in water 10′. O. and H. had $\frac{1}{16}$ of Phos. hydrogen added to it and then the plate put up. It shewed no sensible action even by Saturday after 70 hours. The plate was then put into fresh O. and H. The old gas examined smelt strongly of Phos. Hy. and burnt with a bright phosphorus flame. The plate in its new gas did not shew any direct action, and on Monday 18th at 8 P.M., or 50 hours after, there was no action; nor on Thursday following. Hence the plate has had its power taken away by Phos. Hy.

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SULPHURETTED HYDROGEN 1060. A Plat. plate prepared by Alkali, etc. In S. A. 8′, in water 15′. A tube of O. and H. had $\frac{1}{20}$ of Sul. Hy. (from Sulet. of Antimony and M. A.) added to its contents and the plate put in at 12 o'clk. No action occurred even by Friday the 15; nor by Saturday the 16th at 20′ to 2 o'clk., or after 50 hours. The plate was taken out and put into fresh O. and H. The old gas examined—it was fetid and burnt with a blue flame. There was no action of plate on new gas at first. Nor on Monday 18th, 50 hours after was there action; nor on Thursday following. Hence Sul. Hy. also destroys power of the plate.

IRON HYDROGEN 1061. A Plat. plate prepared by Alkali, etc. In S. A. 8′, in water 15′. A mixture made of 1 vol. good oxygen and 2 vols. of the hydrogen from steam and iron made 7 days ago and which has been standing over water ever since. The plate put into this mixture. No action took place. On Friday the 15th—the plate taken out and put into good O. and H. (hydrogen from zinc), it acted at once and well, just in the usual way. The plate had been in the ferruginous O. and H. 22 hours without action.

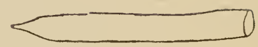
1062. The hydrogen from Iron and steam is now 7 days old—it does not stink as it did and has little or no smell. I think it has diminished a little in bulk over water but am not sure; but it is

worth examining in that respect—there may be a new gas here. It burns *now*, either with or without oxygen, with a purplish flame. It is probable a little carbonic oxide is here and that might account for the non-action, but the colour is rather purple, not pure blue.

1063. Two Plat. plates prepared by alk., etc. In S. A. 8′, in water 15′. Two mixtures made. One of equal vols. of good O and H., and the ferruginous O. and H. used above; the other 3 vol. good O. and H. + 1 vol. ferruginous O. and H. The plates put in at 12 o'clk (Midday). On Saturday the 16th at 2 o'clk., i.e. after 50 hours, no action in either tube. The plates taken out and put in good O. and H. They acted at once—but not quickly. In 1 h. 43′ the water about 1 inch up in first tube and about half up in the second. In $4\frac{1}{2}$ hours, gas $\frac{1}{3}$ up in first and quite up in second.

On Monday Morng. (18th) gas also up in the first.

1064. I tried to obtain some proofs of condensation of gas about platina. A tube of the size[1] and shape figured, contracted at one end, open at both, was stuffed hard with Spongy platina by a wooden [ ], pressed, then connected with a drying tube and a jar of hydrogen and endeavours made to send the hydrogen through—but it would not pass.



1065. A similar tube made and the platina put in lightly, i.e. pressed only moderately. Through this the hydrogen went. It was then closed at both ends, immersed in distilled water to exclude the gas and the rest expelled by boiling. The gas was hydrogen, but did not surpass what the empty tube would have held. But then the spongy platina weighed only grains and would have occupied only part in the solid state of the capacity of the tube. Hence much space not subject to surface action.

1066. Refer action to surface effect entirely—very probable—very like also a specific action on different gases. Perhaps metals Hygrometric to vapour, oil gas, etc. etc. If so, prove a good deal.

1067. If surface action, then have to do with permeation of gases and Graham's and Mitchell's effects. Then his law not true: will be a kind of test of his view or mine to use other bodies, as metals, etc. etc.; for if specific action on bodies, then different bodies give different mixing laws.

[1] This diagram is reduced to half scale.

1068. If there be an action of surface, then also influential in many other phenomena. Query the following. The passage of gases through tubes—the passage of hy. out and air into a jar without the mercury falling: in this case, two bodies, metal and glass, having probably very different surface attractions.

1069. Remember that when a gas rests against a solid, as platina, at least half its elastic power gone on the side of the solid. Not merely a condensation in bulk to one half, but the particles necessary to elasticity by their mutual action, gone.

1070. Prepared some substances for the purpose of observing their hygrometric action. 1. Pulverised rock crystal—boiled in water, washed well in distilled water—allowed to subside—the water poured off, the silica dried and heated red hot in platina crucible. 2. White cornish clay boiled, washed and heated to dull redness in plat. crucible. 3. Per oxide of iron boiled, washed, dried and ignited. 4. Oxide of Manganese which has been heated for oxygen, Do. 5. Spongy platina. 6. Precipitated silver—boiled, washed and heated on sand bath well. These put in separate small basins into a space containing water beneath and covered over, being left for 3 hours. Portions of each were put into clean cold glass tubes and heated. 1 gave water, but not much. 2, clay gave much. 3. Peroxide of iron gave some, between 1 and 2. 4 gave most; looks like an actual compound formed by exposure; must examine this. 5 gave a feeble trace. 6 gave also a feeble trace, more than 5.

1071. On trying to prepare rosin and amber, I found they contained water under common circumstances.

1072. Has the presence of ammonia in oxides of iron anything to do with these effects generally?

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1073. On the 19th I put portions of the same substance[s] ( ), all previously heated, into a jar containing camphor, so as to ascertain how far they would condense the vapour of camphor. All was dry.

1074. To-day examined them. The pulverised rock crystal 1 had hardly a trace of camphor in it. The clay 2 gave much upon heating—the oxide of iron 3 also much—the oxide of manganese

4 very much. The silver 6 some, much more than 1, but not so much as 2, 3 or 4. The spongy platina 5 gave about as silver.

1075. It is remarkable to observe the attraction of all these substances for the vapour of camphor, especially of such as the oxides of manganese, iron and alumina. I think the metals condense more of this substance than they do of water.

1076. I put also a glass tube, gilt in places and in places naked, into a vessel of camphor, and applied slight warmth; on the whole I thought more camphor adhered to the gold than to the glass, but the difference, if any, was not very decided.

1077. It seems as if the idea that the power is abstroud[1] to one half on the superfices of the vapour will explain the effect of nuclei, etc. etc., both in vapours and also in solutions.

1078. Now experimented on different gas with *Spongy platina*, not with Plat. plates, and prepared a portion on the end of a platina wire for the purpose; it was made from very pure ammonic muriate of platina.

1079. The Hydrogen from Iron and steam directed in a jet against the spongy platina. It would not cause its ignition if the platina and also the end of the jet were quite cold, but if either were a little warm the temperature increased and ignition ensued.

1080. 4 vols. of Zinc Hydrogen + 1 vol. carbonic oxide were mixed and directed against the platina. This mixture would not ignite it if all were cold, but did if either were warmed a little first.

1081. Another mixture of 3 good Hydrogen and 1 carbonic oxide did not act—nor did adding 2 vols. of Carbonic acid produce any good effect.

1082. A mixture of 4 vols. Zinc Hydrogen + 1 vol. Olefiant gas did not act when all cold. A mixture 3 vol. hydrogen + 1 vol. olefiant gas—no action on spongy platina when applied as a jet if platina and jet cold, but when platina warmed I thought it acted better than similar mixture of Carb. ox and Hy.

1083. A mixture of 1 vol. Hy. + 1 vol. Olefiant—had no action as a jet on spongy platina when cold—when warmed and cooled still no action—if retained warm, then action.

[1] ? abstrude = to thrust away (*O.E.D.*).

1084. A jet of mixture equal vols. hydrogen and nitrogen acts very well on cold spongy platina, igniting it.
1085. A jet of mixture 1 vol. hy. + 2 vols. air act very well on cold spongy platina, igniting it.
1086. A jet of mixture 1 vol. H. + 1 vol. carb. acid acts very well, igniting the platina. A mixture 1 vol. H. + 3 vols. Carb. acid acted very well—yet mixture would not burn at end of small jet even when lighted at a candle, and scarcely at the larger aperture of the stop cock.
1087. A jet of mixture of 1 vol. H. + 7 vols. carbonic acid caused cold spongy platina to heat and ignite; yet this would not burn at the candle flame, but was actually able to extinguish a taper, though it could ignite cold platina.
1088. On the contrary, the carb. oxide mixture which burns well at the candle has no power of heating the platina, and yet far more of the heating agent, i.e. of the hydrogen in it.
1089. A little ether put into a bladder of Hydrogen gas did not take away its power of heating spongy platina, though it seemed to retard it. It does not destroy the power of the plate, but retarded it.
1090. A little oil gas liquor in hydrogen did not take away its power but retarded it. It does take away the power of a plate unless very well prepared. This mixture burnt far more brightly than that with Oleft. gas, yet the power here remained and there was taken away as to common temperature.
1091. A little sulphuret of carbon takes away power from the hydrogen, i.e. action begins but a sulphuret of platina (superficial) is formed and the action ceases. If the platina be heated redhot by the spirit lamp it cannot be retained so by this mixture, but gradually falls in temperature and becomes quite cold. A spirit lamp will drive off the sulphur from the platina and restore its first state.
1092. Good O. and H. were mingled in the right vols., 1 + 2. One volume of this mixture was mingled with 3 vol. Carbonic acid, a dry tube filled with the final mixture and the spongy platina introduced. It immediately caused combination as a plate would have done and no more; it did not ignite, there being too much Carb. acid to allow of such action. The same mixture sent as a jet against spongy platina caused its ignition.

1093. Yet this mixture, though it will ignite platina, puts out a taper—singular reversion of ordinary effect and singular contrast to carbonic oxide mixture.
1094. Three vols. of the O. H. gas and 1 vol. carb. oxide mixed, a dry tube filled and the spongy platina put in. Here no action—now would a plate have acted? The mixture thrown as a jet against the spongy plat. did not cause its ignition. Yet *this* mixture explodes powerfully by a taper.
1095. Three vols. O. H. gas and 1 vol. Oleft. gas mixed and put into a dry tube with spongy platina—no action—just as with plate. Nor any action when sent as a jet against the spongy platina at common temperatures.

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1096. I have endeavoured to obtain indications of a power in spongy platina to separate gases previously mixed, as for instance Oleft. gas from Carbonic acid, supposing that the different effects were due to the attraction of one and the non-attraction of the other, but have not succeeded. The tube (1065) was connected with the jar and drying tube there mentioned and a mixture of 1 vol. Oleft. gas with 9 vols. Carbonic acid sent through it. The gas was then expelled by distilled water as before and the Carb. acid removed by potash. The unabsorbed gas was about $\frac{1}{5}$ instead of $\frac{1}{10}$ the capacity of the tube with platina, but it did not burn very brightly, and I suspect common air is with it evolved during the solution of the C. A., etc. etc. It is very difficult in these small quantities to exclude the air from the water, etc., so as to get true results.
1097. On taking a small measure of the mixture at once and absorbing the C. A., etc. by potassa, it gave nearly a fifth undissolved, though it ought to have been only a tenth.

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1098. Cleaned five plates by Alkali. Hot Sul. Acid for 5′—water for 10′. Put two away in a little double distilled water in a tube. Other one in a little distilled water (double) in another tube, and other two in a large half pint phial containing the first portion of the double distilled water. This water was coloured greenish,

the other portions were clear. These were all put aside at 10 o'clk. A.M. this day for a fortnight or three weeks.

Decr. 19th. Took one of the two in the tube and one of the two in the phial, put them in O. and H.; acted at once—neither rose to explosion, but think they would if the water in the trough had not been very acid from S. A. and previous operations. In half an hour nearly all gas gone—went quicker with the plate from the tube. Have been put aside 24 days.

1834. Jany. 17th. Put the other three plates into three tubes of O. and H. No immediate action in any—by evening a little action in one. Next morning, the 18th, one nearly at the top—another a little advanced and the third not sensibly gone. This is 53 days from beginning of expt.

1099. Put the crossed ends of a bent platina wire into hot alkali first—into water—into hot Sul. Acid for 5′—into water for 15′; shook them, dried them by spirit lamp and pressed them together in the chops of the pliers at ½ p. 10 o'clk. A.M. Next day at 11 o'clk. examined them; there was *no* adhesion.

1100. Several plates prepared by alkali, Sul. acid for 5′, and water. In this state platina wets very well; the water runs over any part of its surface—it does not run up into wet and dry parts and when a plate is shaken and dried over the spirit lamp, a rod dipped in distilled water wets it freely any where, making wet marks wherever it is drawn. When dry, Naphtha wets it also well, but when the Naphtha dried off, then water not wet it but the liquid runs up in the usual way. If heated red however it wets well again.

1101. Spongy platina also wets well; water runs up into it as into sugar almost and if a little be sprinkled on water, it wets and sinks immediately, not floating like filings of brass or iron, etc. etc.

1102. *Clean* metals therefore probably wet very well—but they soon get dirty by exposure so as not to wet well.

1103. A Piece of polished Quartz did not wet well on the surface—nor did rubbing or wiping even with a wet cloth make it better. But being moistened on one part with strong oil of vitriol, washed,—and left in distilled water 10′ to remove perfectly the sulphuric acid, then it wetted very well at that place, and when wiped dry it easily and readily wetted there again. It was left exposed to the air but out of other contact at 11 o'clk. A.M.

Next day at 11 A.M. This quartz surface would not wet well; the water ran up and left dry places. The air has therefore affected it, dirtying it. Wiping made it far worse; wiping is not the way to obtain a *clean* surface.

1104. *Obsidian* was as quartz; at first it did not wet, but was made to do so by Sul. Acid. It also was exposed to air at 11 o'clk. A.M. At 11 A.M. next day it was as the quartz, the surface having become soiled. Wiping with the cleanest cloth only made it worse.

1105. *Stone coal* did not do so; it did not wet well even after contact with Sul. acid. It did wet well with Naphtha and also oil.

1106. *Carbonate of lime* wetted well on a fresh cleavage. It was exposed to air at 11 o'clk. A.M. It seemed to have suffered also. Split another piece—wets well at first; put by at 11 o'clk. A.M. of the 26th Nov. On the 28th wets well, but very badly after wiping. Wiping the calcareous spar immediately take[s] off its first state.

1107. *Topaz* did not wet well either after sulphuric acid or by Naphtha.

1108. Some of the clean *dry plat.* prepared as above were also left exposed to air at 11 o'clk. A.M. On the next day at 11 o'clk. A.M. would not wet well as before; had been injured; but other plates left in water for the same time did wet well after being shaken and dried. The air plates, if heated dull red in clean spirit lamp flame, *then* wetted again.

Wiping a plat. plate made it very repulsive of water.

1109. Think this a subject of great consequence, for I am convinced that the superficial actions of matter and the action of particles not directly or strongly in combination are becoming daily more and more important in Chemical as well as in Mechanical Philosophy. I have therefore pursued this matter more closely. As illustrations of mechanico chemical action refer in note to Carb. Soda, Phos. Soda, Iodide Mercury, Oil on water.

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1110. Some Mica recently split wetted very well. If wiped with cloth did not wet; this just as with calcareous spar. The difference cannot be due to electric action, because the wetting should

discharge any electricity as well as wiping, and metals suffer by wiping also.

1111. Some Mica just split put away exposed to air at 11 o'clk.

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1112. The mica of the 26th wetted very well to-day, and also after wiping with a clean cloth wetted well.

1113. Now experimented on the polarity of P. and N. poles, and as a reference for the quality of these poles in relation to zinc, etc. etc. made an experiment with the galvanometer first. When in its place, pieces of Zinc and Platina attached to its wires and touching a drop of acid deflected the needle as in the figure. From this deduced relations of the pieces tried.

1114. The plates used as poles were those before described ( ), of platina with platina wires soldered on. The battery used was freshly charged and of 40 pr. plates double coppers, the charge good but gradually diminishing by use. At first the plates were made poles in S. A. 1336.

1115. Three plates made Pos. and other three Neg., for 5′ in S. A. 1336. One of each connected with the galvanometer wires and immersed immediately in a fresh portion of the same acid; the galvanometer was strongly deflected; the P. plate acted as Platinum to the N. plate as zinc. Using the same plates, the P. plate acted as platina to a clean unelectrified plate (cleaned by cork, emery and dilute S. A.) and the N. plate acted as zinc to it, but both these actions were weak. The Plates soon lost of their power with respect to each other; after 2 or 3 minutes I could hardly find traces of it.

1116. The two remaining P. plates were put into distilled water immediately upon being taken from the battery action and the two N. plates into another portion of distilled water. One of each of these was taken out after 6′, connected with the galvanometer and plunged in S. A. 1336. The galvanometer was affected in the same direction as before.

1117. The action on the galvanometer is one strong impulse and then the power seems gone, for the needle in returning vibrates about the neutral or natural position line. A moment after contact the effect is at an end.

1118. The other two were experimented with after having been twenty minutes in the water. They acted in the same direction as before but not so well, i.e. not so strongly; but it is clear that, charged to this height, it can remain 20′ in contact with water. There was no effect after the first contact.

1119. Four P. and four N. plates in S.A. 1336 for 5′, then in distilled water, separate vessels.

1120. One P. and one N. in the S.A. 1336 and touching each other for a moment only before their contact with the galvanometer–after that scarcely able to affect the instrument. The momentary contact had taken away almost all the power.

1121. One P. and one N., the N. plate in the S.A. 1336 for a minute before the P. plate, but otherwise not touching any other plate. On communicating through the galvanometer the direction was the same and the power considerable, but not quite so strong as in first expts.

1122. One P. and one N. plate, after being 8′ in the water, tried against a neutral clean washed indifferent plate. The P. plate acted as Platina, the N. plate as Zinc, to the neutral plate.

1123. The last P. and N. plates, out of water after 10′, put together through galvanometer as in the first expt. They acted in the same direction as before, but only moderately. The power soon goes off.

1124. Three P. and three N. plates in S.A. 1336 for 5′.

1125. Two of the P. plates rinced in distilled water, wiped on a clean cloth–rinced in fresh distilled water–wiped again–put each into a separate portion of distilled, and left there for 5′; at the end of that time they were put into fresh portions of water and the two out of which they were taken examined by Mur. baryta. They both gave traces of sulphuric acid. Sixteen minutes after, the last portions of water were also examined, and the traces of S. A. there sensible but feeble.

1126. Two of the N. Poles were washed–wiped–washed–wiped and put in separate portions of water in the same way. After 16′ there water gave exceedingly minute traces of S. A.

1127. The remaining P. and N. plates were rinced, wiped and put into separate portions of water. After 5′ both these portions of water gave Sul. Acid.

1128. Four P. and four N. plates in S.A. 1336 for 5′.

1129. One P. and one N., directly out of contact with the battery, made to act through the galvanometer—effect very good.
1130. One P. and one N., directly out of contact with the battery, rinced and wiped quickly and then connected with Galvanometer in S. A.—acted even more powerfully than former, but am not sure of equality of circumstances whilst under the battery action.
1131. One P. and one N. taken out of battery connection—not rinced but left in air 5′. Still acted well but not as the former.
1132. One P. and one N. put in water for 10′—then in air for 5′—then scarcely acted. Water and air together seem to injure more than water.
1133. Three plates were heated with potassa fuza—washed—put into hot oil vitriol and then into water for $\frac{3}{4}$ of an hour, to give them clean surfaces.
1134. Three P. and three N. in S. A. 1336 for 5′.
1135. Two P. and two N. into water (separate portions).
1136. One P., directly out of contact with the battery, connected by galvanometer with one of the three cleaned indifferent plates above: it was as platina to the indifferent plate, which was as zinc.
1137. One N. plate directly from battery contact against same neutral clean plate; the latter was now as platina, the N. plate being as zinc. Hence the pole plates are on opposite sides of the indifferent or unelectrified plate.
1138. One P. and one N. out of the water against each other in solution of Potassa—still the def[l]ection was in the same direction and probably as strong as in acid, for plates had been some time in water.
1139. One P. and one N. out of the water against each other in solution of sulphate of soda. The direction of deflection same as before, but weaker because of continuance in water.
1140. Three P. and three N., made so in sol. *Sul. Soda* strong for 5′. Two P. and two N. in water.
1141. One P. and one N. connected at once by galvanometer and put in S. A. 1336, acted as in former cases as to direction, but not stronger than when electrized in acid.
1142. One P. and one N. after 7′ in water acted well.
1143. One P. and one N., after 7′ in water, put each into the S. A.,

but not touching each other, and left thus a minute; then connected by galvanometer wire and acted very well.

1144. Hence seems proved that there is some state of surfaces induced, but it may be perfect cleanness at P. Pole and perhaps alloyage at the N. Pole.

1145. Three P. and four N. in acid solution Nit. baryta for 7′, then in two separate portions of dilute N. Acid for perhaps 10′.

1146. One P. and one N. by galvanometer in S. A. 1336—after 14′ in water—acted as before but feebly.

1147. Two P. and two N. from the dilute N. A. into distilled water, then into two portions of fresh dilute N. A. Then one of each tried at Galvanometer in S. A. gave a little deflection—the other two put into a tube of dilute N. A. *touching* each other to exert their action in that tube; the liquor then tested for baryta but none perceived at once. The two preceeding washing liquors also tested for baryta but none there also.

1148. This expt. was to see if any alloy of barium and platm. which remained until voltaic action, because then set free and found in tube where contact made. But the deflecting action was too feebly—must make expt. again with a better battery.

1149. Remember too how exceedingly minute a portion of matter acting chemically would suffice to produce the deflection observed, and remember also it[s] close adhesion to the platina plate.

1150. Now experimented with plates not prepared by connection with the battery.

1151. A clean plate out of S. A. 1336 against another clean plate out of water; did almost nothing.

1152. A plate heated with Potassa fuza—then in water—if against an indifferent plate was generally indifferent to it but if a little alkali adhering acted as zinc.

1153. A Plate with alkali on it acted as Zinc against a neutral clean plate.

1154. A Plate immediately out of hot oil Vitriol acted against a neutral clean plate as platina would against zinc. Or the plate with alkali acts as prepared N. plate did and that with acid on it as prepared P. plate.

1155. Plates in Alkali and S. A.—then washed well—little or no action against others.

1156. Plates cleaned, some by strong S. A., other by potassa fusa —well washed—tried against each other, did not act sensible.

1157. Either the surface of the poles must be charged with very concentrated alkali and acid or else, as is more likely, there is a real alloy on N. Side.

1158. Is it not possible that at P. side the metal oxygenated? Sometimes the plates from Alkali by heat seemed to act in this way; try this again.

1159. Marianini on difficulty of electricity passing from metal to humid conductors, and vice versa, Ann. de Chimie, xxxiii, 117, 119. Ritter also wrote on this. It relates to force of decomposition and effect of interposed plates, etc. etc. Ritter, Do. Jour. de Physique, lvii, 349, 350.

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1160. Marianini on the source of electricity in piles. Annales de chimie, xlv, p. 28.

1161. The Phenomena of Ritter, de la Rive, etc. etc. must be connected with the agitated mercury of Davy, Herschell, etc. etc. Must experiment and examine that in relation to it.

1162. The metals which are best are just those which shew my effect (due to cleanliness) best.

1163. The effect is always by passage of Electricity from or to *decomposing* bodies, not from bodies suffering no change.

1164. The change is merely that of increasing platina power at Pos. pole and diminishing it at N. pole.

1165. A piece of neutral platina can act with either, yet it is supposed to have no structure or charge. Why should they have therefore?

1166. To say that P. pole acts like a Neg. Metal and Vice Versa is merely to say that Platina has its power exalted at P. pole and diminished or hidden at N. pole.

1167. Consider the transmission of Electricity; that there are three modes as in a metal wire

in decomposing fluids

through air, vapour, etc. as spark or brush.

Are not these all one?

1168. Consider the quantity heat evolved in these three cases and what it indicates.
1169. Is heat evolved in decomposition, i.e. would more heat be evolved in three decompositions than in one, if the current were through the same length of fluid in both cases.
1170. Priestley was probably the first who put forth the view that Electricity is an important agent between mind and body in the animal system—R. Phillips.

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1171. Proceeded to decompose dry chlorides, oxides, etc. etc. to ascertain if there also the decomposition was definite and what the equivalent numbers would be.

1172*. A tube of green glass A had a wire fuzed into the bottom as in the fig. The wire was of platina: a part was coiled up into a ring just in the bottom of the tube and the weight of it was 6·44 grains.

1173. The tube was slung by a platina wire twisted round it to a retort stand and the wire connected with a volta-electrometer with double plates, as in the figure, and that again with the N. end of a Voltaic battery. Proto chloride of Tin was first fuzed in a platina capsule and then put into the tube A. Being then retained in a fuzed state, a platina wire P connected with the Pos. end of the battery was dipped in and decomposition ensued. Deuto chloride of tin passed off from the P. Electrode and Metallic tin was rendered at the N. Electrode. The action went on very well. The vessel remained tight, and the plat. pole, though a little dulled, did not seem to have been acted upon so as to cause any solution.

1174. When the Volta Electrometer was filled with gas (48·5 vols.), the action was stopped and the tube with its contents allowed to cool. The gas in the Volta Electrometer = 2·05 c. inches at 56° and 29·7 Barometer.

Now 100 *c: i: of O. and H. in the prop. to form water* = 12·92 *grains* at 60° *F.* and 30 *inches* Barometer.

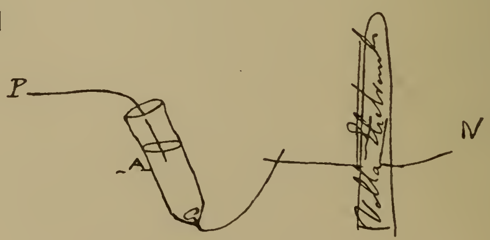
Hence the 2·05 c: i: = 0·26486 of grain of water decomposed.

1175. When the tube was cold it was broken up and the tin was found beautifully reduced and melted into a knob of alloy on the end of the Plat. wire, which with it could be easily separated from every particle of glass and chloride. That being done it was weighed, and now the weight of the Platina and tin together was 8·2 grains. Hence 1·76 of tin had been electro-chemically evolved at the *exode* and of course a corresponding proportion of chlorine at the *cisode*, which had formed *bi-chloride* of tin as a secondary result.

W T

Now 0·26486 : 1·76 : : 9 : 59·805, the tin.

* [1172]



1176. The number for Tin is given 58, which is very near indeed for a first expt. and shews that the Electro chem. equivalent is the same as the Chemical equivalent here. The excess of tin above 58 is probably due to the deficiency of gas in the Volta Electrometer from absorption, etc. etc.

1177. It is probable that a better volta electrometer would be to act on a chloride, as lead or tin, etc. and estimate the electricity by the weight of the latter. Or even humid solutions might be employed; thus the lead or Tin or copper thrown down from a solution would, if washed, dried and weighed, do as well as the volume of gas and be free from the objection of absorption, *for probably all these precipitates are electro chemical equivalents* in their purest condition. Then indeed have an admirable means of comparing numbers of definite proportionals, etc. etc. etc.

1178. Now tried the Value of a plumbago Electrode in acting upon chlorides. It answers beautifully. In fuzed chloride of lead, for instance. Lead was reduced at the N. Electrode and gaseous chlorine at the P. plumbago Electrode. There seemed to be not the slightest chemical action on the plumbago, but some minute particles were disengaged from it and floated about; but these could do no harm, for the carbon had no power of reducing the chloride—were not attracted to the N. Electrode and if they went there could do no harm. Plan excellent for chlorine evolutions. The chloride of lead when cold was not at all discolored, so that no iron or platina or other matter dissolved.

1179. A piece of plumbago made to connect the Electrodes is very powerfully heated. It seems to me to be like Sulphuret of Silver. Would be worth while knowing whether it is a better or worse conductor when heated.

1180. The crystd. Hydrated chloride of Tin is very volatile when heated. A case of this kind occurs with hydrated Boracic acid, where volatility is given by the water. Hydrated chlorides an interesting class of bodies. Query in a hydrated chloride, as of Tin, does the Chloride or the Water or both give way Electrochemically.

1181. Made another experiment upon the Chloride of tin as before, but continued the action until the Volta Electrometer had been four times filled with gas. The quantities according to its graduation were 53—54·5—55—54 vols., and these put together and measured thrice gave a result of 9·13 cubical inches. The Platina wire at first weighed 4·29 gr.; afterwards with the tin it weighed 11·75 grs. The tin therefore = 7·46 grains. The button of alloy was very fine and well melted.

1182. The gas, being 9·13 c:i: at 57° F. and 29·5 inches mercury pressure, would = 1·1796 nearly, i.e. if 60° F. and 30 inches be allowed to stand for the above, then 1·179596 : 7·46 : : 9 : 56·833 (W T). Here the result in Tin is too light, but there are plenty of reasons for that, and it is abundantly sufficient to shew the definite nature of the action of the Electric current.

1183. Experimented with *Chloride of lead* in the same way exactly, for that chloride can also be fuzed by spirit lamp in the tube. The platina wire used in the tube was thin but long; it weighed 9·6 grains.

1184. Whilst the Volta Electrometer was filling for the third time an effect took place which obliged me to stop; the alloy of lead with Platina is so fuzable that the wire had dissolved away in the tube until the *end* only touched the melted globule above and the aggregation or some other circumstance caused the globule to separate from the wire, leaving a film between so that the spark passed there. The action was therefore stopped and the results estimated.

1185. The gas in the Eudiometer = 50·5—56·0—and 7·3 vols.; measured twice by the small c:i: measure it amounted to 4·92 c:i:, but by another measure to 4·87; the latter however not a delicate graduation. Temp. 56°—Bar. 29·5.

The button of alloy was very good but very like bismuth in colour, brittleness and crystalline appearance of fracture; it with the wire = 16·7 grains. Being dissolved in N. A. it gave a solution of lead and left platina. The lead must have weighed 7·1 grains.

The 4·92 c:i: = 0·635664 gr.

W L

Now 0·635644 : 7·1 : : 9 : 105·11

The equivalent of Lead is 103·5 or 104. Hence this result as near as could be expected and very good.

1186. Again with *chloride of Lead*, using a much thicker wire in the tube, it weighed 18·6 grains. All went on well; the chlorine at the P. Electrode bleached litmus instantly and the chloride of lead at last as white as at first. The Volta electrometer was six times filled with gas as follows by its own graduation—55—54·5—54·75—54·5—54—54·5. These put together and measured gave 13·65—13·68—13·77—13·65, the mean being 13·68 c:i:; these = 1·767456 gr. water decomposed.

1187. The alloy of lead broke off from the wire as before, being brittle; it was boiled in water to remove the last remains of chloride. It and the wire weighed 37·7 grains and the LEAD alone therefore 19·1 grains.

W L

Now 1·767456 : 19·1 : : 9 : 97·26

Here it is too little, but still so near as to establish the principle of Electro-chem. equivalents.

Various.

1188. *Proofs against De la Rive's theory.*

Ⓟ Sul. Soda—Nit. Soda—Nit. Baryta Ⓝ

S. A. collects here and *No* N. A. — Baryta collects here and *No* Soda.

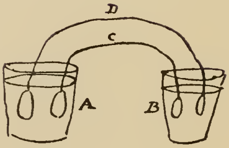
again

Ⓟ Nit. Baryta—Nit. Soda—Sul. Soda Ⓝ

No Sul. Acid here. — *No* Baryta here at first;

but there should be instantly according to De la Rive's theory.

1189. *As to source of Electricity.* A contains an acid strong or weak; B a neutral solution strong or weak; C consists of a zinc wire with a zinc disc at each end; D of a platina wire with a platina disc at each end. If electricity *originates* from chem. action it is very strange that none is produced in this case, i.e. that weak brine can evolve as much as strong acid, etc. etc. See results of 19th Feb. 1834.



1190. In respect to this matter of Source. CONSIDER WELL the state of a Single pair of plates *before* and *after making* and *breaking* their mutual contact.

1191. The present voltaic apparatus, i.e. the trough, must be a very coarse wasteful arrangement if referred to its first principle. For the Zinc dissolved *ought* to supply electricity enough if rightly collected to affect the world almost.

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1192. With regard to INTENSITY and its meaning, etc. Define intensity if possible and state its relation to *quantity*, *time* and *conducting power*.

1193. Its relations in wires—solutions, etc. etc. having different extensions and diameters, etc. etc. very curious; also with reference to decomposing actions going on or not. Thus in diagram*

the intensity at 1, 2 compared with that at 5, 6
" " " " 3, 4
" " " even with that at 7, 8 in the continuous wire.

Then again, making 9 and 10 large plates or small or varying otherwise whilst one constant current of Electricity runs through the whole.

1194. Investigate the intensity by decomposition test or by galvanometer test, etc. etc.

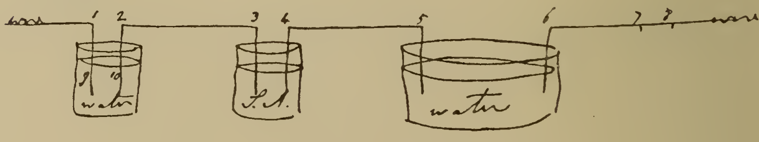
Nervous agency of Electricity.

1195. Each leaf of a tree subject to electric currents—each has its cisode and exode, the latter in the roots. The same true of the rootlets. Hence cause for direction of sap, etc. etc. Perhaps cause why shoots, etc. upwards, why roots downwards, according to the matters evolved at cisode or exode, or tendency to evolve there. Cause perhaps why *ends* of shoots in Spring put forth leaves first and most strongly and from these why trees grow in range and extent.

1196. Probably be able to invert the growth of a seed, as a bean. Ask Lindley for a quick growing seed; put it into a vessel with reflector, etc. etc. so as to have equal light; then determine an electric state of the air by Machine and Leyden Battery as store house, making the seed and its [illegible] the discharge. Can easily determine the current of Electricity in any given direction.

1197. Query the effect of lightning rods in vineyards; more like

* [1193]



from natural reasons to injure vegetation than to help it. See on this what Mr Madden says in his *Infirmities of Genius*, Vol. 1.

1198. As action of Electricity *definite* whether viewed Magnetically or chemically, then probably definite in quantity of action in vital functions. Then how minute a quantity affects frogs and how little required for the mental Government.

1199. Then more would do harm, and probably the conducting power of Nerve and Muscle are so near alike that no improper accumulation can take place; the lateral conduction being the safety valve or regulator of the animal Economy in that respect. So that man can not hurt himself casually, although he has so much nervous power at command in Machine, etc. etc. This inefficiency consists in his not having the conducting system at command also.

1200. Consider the Torpedo also in relation to these points and its insensibility to its own electricity, whilst it suffers from the Voltaic current sent through it. Could probably combine matters as to make the Torpedo shock itself.

1201. ELECTRO CHEMICAL EQUIVALENTS, propositions relating to.

1202. A body which is not decomposable alone by Electric current is not *directly* decomposable when in combination, as for instance Boracic acid is not alone—therefore not in combination except by secondary action. Water, Mur. acid are decomposable alone and are also in combinations, though not essentially so. It is only to decide whether, if not alone, it is not in combination, and Vice Versa, for the principle would then settle many substances not subject to action otherwise.

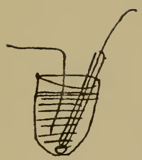
1203. The nature of the Pole makes no difference in the definite decomposition if it have no power, not being part of the electric circuit. Therefore may get weight of principles in that way instead of collecting them—thus Chlorine by quantity of chloride of silver formed and Fluorine or iodine in same way.

1204. When a body of known composition is decomposed directly by the Electric current, may conclude that its proximate principles are *Electrobeids*, and may deduce their numbers from their chemical equivalents, for the elements of that body which is directly decomposed must be equivalent.

1205. Does it not seem to be an axiom almost that, when in a case of true Electro chemical decomposition an Electrobeid is rendered at one Electrode, there must be a corresponding Electrobeid at the other?

1206. Is it not true also that bodies which, being made Electrodes, can combine with the substance evolved without being themselves changed in composition, are also Electrobeids and combine in Electro chemical equivalents?

1207. In the table I mean Real Electro chemical equivalents, not hypothetical; for we shall else outrun fact and lose the information directly before us. It does not follow that all bodies which combine should be in the combining proportions. Electro chemical equivalents: I must keep my researches really *Experimental* and not let them deserve any where the character of *hypothetical imaginations.*



1208. *Practice of decomposition.* I can decompose chlorides, iodides, etc. requiring a strong heat thus—the negative wire being enclosed in a tube but sealed to it below. May so act on fuzed salts perhaps also.

1209. Act on Chlorides of Sodium, Potassium, Barium, Strontium, etc. etc. by a globule of tin at N. Electrode so as to cause their fusion, and collect them safely so as to weigh them.

1210. May obtain the loss of chlorine perhaps by loss of weight.

1211. Decompose chlorides of Barium, Magnesium, Calcium, Strontium, etc. etc. by Platina for the metallic bases. Probably obtain them crystallized.

1212. Search for Fluorine by using a plumbago Pos. Pole acting on a fluoride.

1213. This process may finally give rise to some very good processes of analysis in determining weights, or at least to some excellent modes of comparing weights of metals. Thus by submitting vessels containing Chlorides of lead, tin, etc. etc. to same current, get the comparitive quantities very accurately—*a good principle of analysis*, for it will hold probably in salts as well if properly selected, and may use mercury electrodes when convenient.

1214. *Chloride of Antimony.* I wanted to obtain the equivalent of Antimony from the chloride or sulphuret, but have been a little embarrassed. Ordinary sulphuret of antimony was digested in M.A. pure in expectation of obtaining a solution of proto chloride of antimony, and from it both the proto chloride and protoxide. Sulphuretted hydrogen came off as usual, etc., but in filtering the solution there was occasionally appearances of a little sulphuret or sulphuretted oxide in the solution. However, I do not think it made any final difference.

1215. A part of this solution was precipitated by abundance of water, then well washed with water, next with weak solution of carbonate of Soda, and then again with water and filtered—dried on chalk stone and so prepared for use. It was of a buff colour, but I thought that might be in part due to the hydro sulphuret present.

1216. Another part of the solution was evaporated; it gave off much fumes of M.A. Being put into a platina crucible and evaporated still more, I could not get it to solidify on cooling. It was very volatile, and in successive portions the whole evaporated without its having at any time acquired the condition of becoming solid by cooling. Whether this is due to the presence of water not separable or to other circumstances, as of composition, is just now rather doubtful to me.

1217. I then made some chloride from Metal antimony and corr. sub. having the usual qualities.

1218. Compare these two chlorides again.

1219. The latter chloride was put into a tube connected as before with a Volta electrometer, and all arranged for its decomposition, a piece of plumbago being used as the P. *Electrode.* But decomposition would not proceed. Hence this body not decomposable by Electricity.

1220. When the two Electrodes very near each other in the chloride there was a little decomposition and a very little gas evolved in the Volta Electrometer. But I incline to attribute this now to the little water present, which it is almost impossible to avoid. This will account for the effect produced under the new law of conduction, but I suspect that in all true cases the change from non-conducting to conducting is far more striking, and that

in the other feeble cases water or some other decomposable body had been present, producing effects not due to the body at the time thought to exhibit them. Still the fact as stated in the former paper is true because *true of the water*, etc. etc.

1221. *Sulphuret of Antimony*. Wishing for the power of decomposing sulphuret of antimony by the E. current so as to make out Antimony and sulphur as *Electrobeids* and get their *Electro chemical equivalents*, I proceeded to ascertain whether I could fuze sulphuret of antimony over platina out of contact of air without injuring the metal, and also whether I could do the same with the sulphuret and the metal antimony, for if not there would be no chance of collecting the metallic result at the *N. Electrode.* A piece of platina was therefore fuzed by a spirit lamp in a glass tube under ordinary sulphuret. When cold and examined, the platina was gone and the tube contained two substances, both black and solid; the bottom heavy portion was very glassy and brittle, the upper portion was the ordinary crystallized sulphuret of antimony. The platina quite gone.

1222. Put antimony and sulphuret of antimony into a tube and fuzed over a spirit lamp: when cold examined. At bottom metallic antimony, well divided from substance above, but much smaller in quantity than when put in. Above it *a sulphuret* of antimony, not crystallized but very glassy, and breaks with glassy fracture, very dark indeed. When fuzed, transparent in thin films. This evidently a new sulphuret of antimony containing less sulphur than the common crystallized sulphuret.

1223. Made a rough expt. on proportions: 1056 gr. pulverized Sulphuret antimony and 700 grains pulverized metal fuzed together in a crucible. On taking it out of fire the crucible cracked and part of the sulphuret ran out, but I saved I think all the metallic antimony below. The button when cleaned, etc. weighed 298 grains. Hence 402 grs. had been combined with the sulphuret.

Now 1056 contain already 771·2 metallic antimony
but they took 402·0 more. This approaches to about one half more of metal in this new sulphuret.

1224. It is evident this low sulphuret may exist more or less in ordinary sulphuret, for if sulphur burnt off, this formed. At the same time they separate on fusion from each other. Are therefore

very distinct bodies. This sulphuret may be present in large quantities in glass of antimony. Make glass directly with it.

1225. The new sulphuret is generally glassy, but if cooled slowly becomes somewhat granular, perhaps even at last crystallizes. There was the appearance in the cavities as if so. I think it is more fusible than the ordinary sulphuret—transparent in thin films when fuzed—very liquid. When solid does not conduct battery current. When liquid . A platina wire fuzed under it.

1226. When digested in Mur. acid gave sul. hydrogen gas and a very good solution, which in water precipitated quite white. A very little powder was left insoluble and this was metallic antimony.

1227. The chloride here present most likely not the same with that from common sulphuret. Nor should the oxide be the same. The chloride probably the same as that from Metal Antimony and Corr. Sub., but if so, then the oxide, not protoxide; yet soluble in M. Acid, etc. etc. Examine these two chlorides and oxides and see how they relate to the acknowledged oxides and compounds.

1228. *New Volta Electrometer.* Have been using a new form of instrument, thus*—the Electrodes in the wide part of the tube, the other part drawn out and bent as a neck and put under a jar. When in use the gas evolved passes into the jar and the operation, whether long or short, can go on at once, which is important in furnace operation, and the Volta Electrometer is not disturbed each time—answers very well.

1229. *Electrobeids and Electrochemical equivalents.*

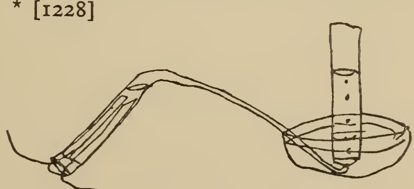
1230. If an Electrobeid present it will not go to one Electrode unless it be combined with another Electrobeid which can at the same time go to the other Electrode.

1231. As a consequence, if combined with another Electrobeid of the opposite class, it will decompose.

1232. As a consequence, when a body is decomposed, two electrobeids are present.

1233. As a consequence, only one compound of the same elements, proximate or ultimate, decomposable; for two compounds cannot exist both of which contain *Electro chemical equivalents*—

* [1228]



and it appears that only single Electro chemical equivalents, *not multiples*, can go to the Electrodes.

1234. The *Electro chemical equivalents* always consistent, i.e. not one number for A with B, and another for A with C; B and C being always taken as Electro chemical equivalents.

1235. Whether all Elementary substances are Electrobeids I cannot as yet tell. Whether they all hang together as one series or whether there are two or three series I cannot as yet say. In fact there must be two or more series, for Sulphuric acid is an Electrobeid, but not matchable against a metal. The compound Electrobeids probably simple combinations of Elementary Electrobeids. Yet this cannot be, for Sulphuric acid is an Electrobeid, yet not compounded of *Electro-chemical equivalents*. Again, Nitrates and Nitrites would probably both decompose, as also Phosphates and Phosphites, yet the acids, though subject to the current as Electrobeids, not all resolvable into Electrobeids and E. Chemical Equivalents.

1236. Is it possible that there should be two electro chemical equivalents for the same body, thus—will 8 of oxygen separate from combination with one body and 12 of oxygen from combination with another?

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1237. Decomposed *Iodide of Lead* in a tube, but the tube was covered and strengthened by an outer jacket of platina foil and the Positive pole was of plumbago. All proceeded well and iodine came off very beautifully.

1238. The plat. wire or neg. Electrode weighed at first 15·4 gr., and after the action it weighed 26·2 gr.—hence the lead separated weighed 10·8 grains. The lead button, etc. was very good and well melted together, looking just like the other alloys of lead with platina. The remaining iodide was unchanged in its appearance. The gas received was equal to 9·97 cubic inches at temp. 53°. Bar. 29·5.

The 9·97 = 1·288124 gr. of water and

$$\begin{array}{cccc} & \text{W} & \text{L} & \\ 1{\cdot}288124 : 10{\cdot}8 & :: & 9 : 75{\cdot}459 \end{array}$$

The Lead here very wrong—is the expt. a bad one or is there a sub iodide of lead produced?

1239. *Chloride of Silver.*

Weighed a silver wire for a positive electrode: it equalled 18·4 grains. Melted Chloride of silver in a piece of glass flask and sent the voltaic current through it and a volta Electrometer, using the above and another silver wire as the communication and withdrawing the Negative electrode as the silver was reduced there; whilst the P. Electrode was put further in as it dissolved away.

At the end of the expt. the gas collected = 9·18 cubic inches, temp. 54°. Bar. 29·56. The Pos. electrode wire was diminished to 6·59 grains; hence 11·81 of silver had been dissolved there.

The 9·18 c: i: = 1·186056 gr. water decomposed.

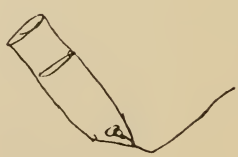
$$\text{Now } \overset{\text{w}}{1{\cdot}186056} : \overset{\text{s}}{11{\cdot}81} :: 9 : 89{\cdot}616$$

The silver too small, but there is every chance that sometimes the small slender crystals should have made contact under the surface of the chloride. Is a difficult expt. to make.

1240. The silver with the chloride at the N. Pole put into caustic ammonia to redissolve the latter and give weight of the former; it amounted to 10·9. Hence Silver filaments or crystals had been left in the liquid chloride and therefore probably had now and then conducted.

1241. The *new Sub sulphuret of antimony* fuzed with a piece of platina wire in a glass tube—is very nicely liquid at a red heat. When cold examined—*the platina remained*; it was a little brittle perhaps but still it bore bending to and fro many times before it broke. Its outside was rough and the sulphuret seemed to adhere strongly to it.

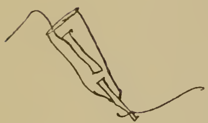
1242. Subjected this sulphuret to action in a glass tube coated outside with platina foil. The Positive electrode was of plumbago: the negative one of platina and weighed at first 13·6 grains. When melted the substance conducted pretty well and action went on regularly. Decomposition seemed to take place; there was a smell of sulphur at the Positive Electrode and corrosion of the platina seemed to occur there. I was afraid of going on too long because the sulphur at the P. Electrode did not separate entirely, perhaps only very little, but combining with the sulphuret would make it



ordinary sulphuret and thus confuze result; for that sulphuret could pass to the N. Electrode and prevent metal being separated there. Only 3·29 c:i: of gas evolved in Volta Electrometer.

1243. On opening the tube there was no regular button; the platina had disappeared at the N. Electrode and the part there was different to the rest, but it was very brittle, black, etc. and could not be separated. Tried to separate the sulphuret by Mur. acid but could obtain no good result.

1244. Then experimented on the *Ordinary sulphuret of antimony*, using a platina N. wire and a plumbago P. Electrode. The action went on when fuzed much like the last, and 2·85 c:i: of gas was obtained. But when examined it was found that the platina had given way below (as was to be expected from the mere effects of the heat on the substance) and that some sub sulphuret had been there formed. The results therefore very confused.



1245. Now tried the ordinary sulphuret with both electrodes of plumbago, not to collect results but see if it would decompose, etc. The plumbago was easily fuzed into a glass tube. When fuzed the current passed, but not when solid. Such is the case also with the other sulphuret. Whether it decomposed or not I cannot say, for though there was a feeble appearance of combustion of sulphur at the P. Electrode, yet I was not sure it was electro chemically evolved. When broken up, there was no appearance distinct of metallic antimony at the N. Electrode, but the sulphuret there looked a little changed and glassy. It was very difficult to separate from the surrounding matter.

1246. Will the other sulphuret decompose with carbon poles?

1247. If decomposition has occurred in these two cases then two different proportions of the same body must have gone to one of the electrodes. Query, which is the constant body, Sulphur or Antimony.

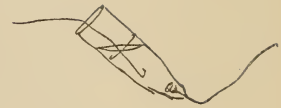
1248. Does not this also look like uncertainty of decomposition?

1249. But then Sulphurets are curious bodies. There is for instance the Sulphuret of silver. Besides which Sulphur is a curious body and goes to both poles. Associated with this may [be] water, its separation from acid or alkali and its power of going to both Electrodes.

1250. Now tried to experiment with oxides but found it very difficult.

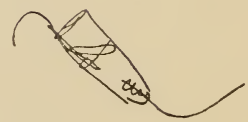
1251. Tried *protoxide of lead* obtained by decomposing and fusing nitrate of lead in a green glass tube cased in platina foil and heated in a charcoal furnace.

The platina wire weighed 14·8 grs. The heat required was very high and the oxide fluxed the tube and issued out, so that I had to stop the experiment when only 1·46 c: i: of gas in Volta Electrometer. These were of no use, for on examining the tube there was no chance of obtaining the metal reduced; the alloy had run out along the platina. The decomposition went on well whilst it lasted; oxygen came off well and the Platina at the P. Electrode not affected.



1252. Tried *Borate of lead*, but it froths so much I was obliged to stop. Must make the expt. in an open vessel, not in a tube.

1253. *Protoxide of antimony* in a tube as in former cases coated with Platina foil and heated in a crucible furnace. Both Electrodes Platina. The oxide fuzed very well and the action went on very well at first, but soon diminished and at last almost stopped, though only 0·5 of c: i: of gas obtained in Volta Electrometer. There was no appearance of gas at the P. Electrode and I concluded that per ox. ant. had formed there as a secondary product which, being infusible, had stopped the electric current.

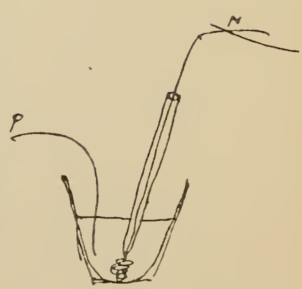


On taking out the wire I found this was the case; it was thickly encrusted with a coat of per oxide apparently crystallized. This put into Mur. acid allowed a little prot. oxide to dissolve and the per oxide was left untouched.

1254. At the N. Electrode a little antimony had been reduced, but it was too small in quantity and the wire too brittle to allow of its collection and weighing.

1255. The *oxide of Bismuth* requires too high a heat and is too powerful a flux to allow of an experiment.

1256. I now worked on *Borate of lead* and oxide of lead, using small porcellain crucibles which I had had from M. Mitscherlich, and surrounding the N. Electrode by a jacket of fuzed green glass made out of a tube, arranging all thus and using a charcoal fire in crucible furnace to melt the substances. The process on the whole went on very well, though the froth of oxygen at P.



Electrode, by enveloping the metal, prevented quick action. The action gradually decreased, perhaps from failure of battery, etc. etc. in strength and other causes.

Succeeded in obtaining 1·96 c: i: of gas in Volta Electrometer. The N. wire originally weighed 20·8 gr. and with the metal weighed 23·65 gr., hence lead reduced = 2·85 grains. Now 1·96 c: i: of gas = 0·253232 grs. of water and

$$\begin{array}{cccc} & & W & L \\ 0{\cdot}253232 : & 2{\cdot}85 :: & 9 : & 101{\cdot}29. \end{array}$$

This is not far off and looks very well.

1257. Worked with the oxide of lead in the same way. The wire weighed at first 32 grains, and after the experiment 36·28, hence 4·28 grains of metallic lead. The gas in the Volta Electrometer was 3·2 c. inches and these are = 0·41344 gr. water, so

$$\begin{array}{cccc} & & W & L \\ 0{\cdot}41344 : & 4{\cdot}28 :: & 9 : & 93{\cdot}17 \end{array}$$

This is much too little for the lead, but I fear that from the circumstances of the expts. I may have made the P. Electrode touch the N. Electrode now and then under the surface of the litharge.

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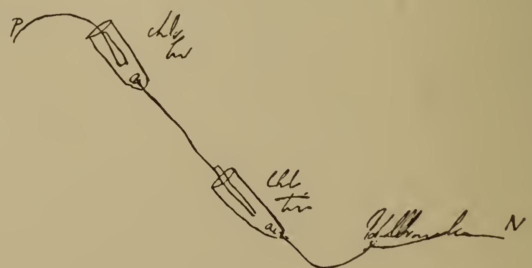
1258. Arranged a nice Expt. to-day with *prot. oxide of lead* in Porcellain crucible, good poles, etc. and plenty of oxide to prevent chance of touching metals beneath. But failed, for the heat required is so high as to make the oxide flux the crucible, and it ran through. It is almost impossible to heat enough litharge and yet to save the crucible—if at least the expt. be continued a reasonable time.

1259*. Made an expt. to-day passing the Electric current at once through Chloride of tin, Chloride of lead and the Volta Electrometer. Both these were in tubes of green glass. Each was heated by a separate Spirit lamp. The negative Electrodes were platina coiled up at the inner ends. The positive Electrodes were both plumbago, though that was not needed but rather an inconvenience with the tin chloride. The results were as follows.

In the volta electrometer were 3·85 c: i: of gas = 0·49742 grs. water.

*[1259]



1260. The tin N. Electrode weighed at first 20 gr. and afterwards 23·2 gr.; hence 3·2 grains of tin had been separated.

W T

Now 0·49742 : 3·2 : : 9 : 57·9

This is exactly the number in Turner for Tin.

1261. The lead Electrode weighed at first 18·95 gr. and afterwards 23·4, making the lead = 4·45.

W L

Now 0·49742 : 4·45 : : 9 : 80·51

This is very short for lead, but I fear I must have touched the metals beneath. This could well be, for as I held the wire dipping into the chloride of lead, it wavered with my hand. But the wire descending into the chloride of tin was a fixture. Try this and the iodide again.

26 DECR. 1833.

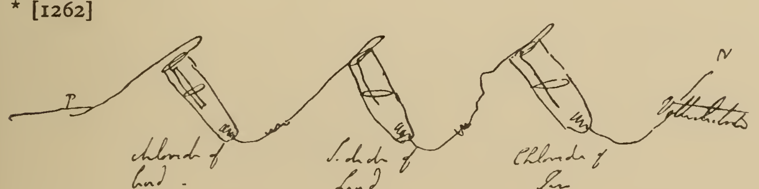
1262*. Made an expt. on the simultaneous and successive decomposition of *Chloride of lead*, *Iodide of Lead*, *Proto chloride of Tin* and *Water* in the Volta Electrometer. Had a new battery and fresh charge that all might be well. Arranged the substances in the following order. The P. Electrode in the chloride of lead was plumbago. The other two platina. All were bent at the top so that they could be stopped by the mouths of the tubes and insure a certainty that contact did not take place between the electrodes in the liquids. The whole was arranged permanently before hand and the liquids were retained hot and fluid by a spirit lamp under each. No platina jackets are required for these substances.

1263. At the beginning the three negative Electrodes were weighed:

the larger =	26·7 grains	and was used for the	chloride of lead.
middle =	23·45 „	„ „	iodide of lead.
shorter =	23·8 „	„ „	chloride of tin.

1264. All went on well except that the vapour of bi chloride of tin in the third tube brought up with it as froth very much of the proto chloride. It ran over and the quantity within was so much diminished there was but little contact, besides which, if more were added, so much froth was made about the P. Electrode as to prevent contact. This substance should be decomposed in a wide tube. However, all went on well and at last

* [1262]



the expt. stopped because plenty of gas was collected. The Barometer was 30·18 inches, the Thermometer 50° F. The quantity of gas 10·29 c. inches.

Now 10·29 c: i: of gas = 1·329468 grs. water.

1265. The *chloride of lead* wire after the expt. weighed 41·5. Hence the Lead weighed 14·8 grains.

$$\text{Now } 1{\cdot}329468 : 14{\cdot}8 :: \overset{\text{W}}{9} : \overset{\text{L}}{100{\cdot}19}$$

The lead here too little but still approximates well.

1266. The *iodide of lead* wire after the expt. weighed 34·3. Hence the lead 10·85 gr. only, not near 14·8.

$$\text{Now } 1{\cdot}329468 : 10{\cdot}85 :: \overset{\text{W}}{9} : \overset{\text{L}}{73{\cdot}45}$$

This very short and was also very short in the former case of iodide of lead.

1267. The *chloride of tin* wire weighed 23·8 grs.[1] after the expt.; hence the reduced tin 8·8 grains.

$$\text{Now } 1{\cdot}329468 : 8{\cdot}8 :: 9 : \overset{\text{T}}{59{\cdot}57}$$

This is rather too much for tin, but not far off.

1268. In order to ascertain if a sub iodide of lead formed at N. Electrode to account for the loss of lead, fuzed 11 grains of lead under fuzed iodide of lead; but the lead suffered no loss in weight.

1269. Whilst fuzing iodide of lead (crystallized) iodine rises until all is fuzed, and for a moment or two afterwards. I think there is a per iodide formed which is soluble in the protiodide and not quite instantaneously decomposed by dull red heat. The well fuzed protio[di]de, when pulverised and boiled in water, gives a colourless solution and the yellow crystals. A little iodine rubbed with the well fuzed iodide in powder.

1270. *Iodide of lead.* Decomposed it as before in a tube but used a Plumbago P. Electrode and sustained the heat at redness the whole time, so as to avoid as much as might be the formation of a per iodide, which should mix with the rest and re-act upon the lead separated. All was arranged so that the P. Electrode should be steady and never touch the lower N. Electrode.

[1] The negative electrode weighed 23·8 grs. (see Par. 1263); hence this figure appears to be incorrect.

1271. The Platina wire at first = 24 grains, and after the expt. 29·1 grains; the reduced lead weighed therefore 5·1 grains. The gas = 3·99 c: i: = 0·515508 grains of water decomposed.

W L

Now 0·515508 : 5·1 : : 9 : 89·04.

This is too small for lead, but it is more than on former occations with iodide, probably because of proportions taken. The Numbers before were 75·459 and 73·45.

1272. Rubbed a little iodine with some of the fuzed iodide of Lead in a mortar. It appeared to combine with it. And indeed know that a per iodide exists. I have little doubt that it is this and the contact of the bubbles of vapour which cause the effect.

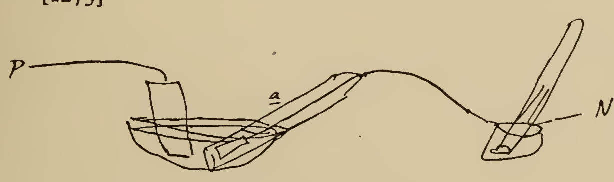
1273. It is wonderful to observe how small a quantity of a compound body, water for instance, is decomposed by a certain portion of Electricity. *One grain* for instance by a powerful current continued for some time. Yet here the relation of conduction and decomposition so close that [it] is probably a true result and the only kind of true result as yet supplied by expt., and it is most likely true that if all the Electricity which acts when the Elements of a grain of water unite could be brought into an available current, it would equal the current required for its decomposition. *Enormous.*

27TH DECR. 1833.

1274. Expts. to ascertain if varying the nature of the Electrode would vary the proportion of substance decomposed by the Electric current.

1275*. Dilute Sul. Acid, about 1 oil vitriol to 3 water, so as to act strongly on Zinc, was put into a basin. An Electrode of Platina *a* in a tube filled with the same acid and inverted in the basin. The end of its wire was connected with a Volta electrometer and that with the Neg. end of a voltaic battery of 40 pr. of plates. A plate of Zinc was connected with the Positive end of the battery and plunged in the same acid. In this case the acid was so strong as to evolve plenty of hydrogen at the Zinc, both when out of contact and also *in contact* with the P. Pole of the battery. Yet this did not in the slightest degree disturbe the proportion of water decomposed by the electric current, for the

* [1275]



gas O. and H. in the Volta Electrometer = 1·53 c: i: and that in the Neg. Electrode tube *a* = 1·04 c: i:, or just 2/3rds. It was of course the Hydrogen alone; the Oxygen had passed to the zinc.

1276. The same expt. was made in a mixture of 1 Mur. acid with 2 water. The acid was strong enough to evolve hydrogen at the P. Zinc, both in and out of contact with the battery. Yet the Volta Electrometer gas was 2·08 c: i: and the gas in the tube *a* 1·41 c: i:, or just 2/3rds. No influence therefore here of the chemical affinity at the poles over the Electro-chemical of the current.

1277. Now used a Pos. Silver Electrode in the Mur. acid. On passing the Electric current no chlorine evolved, but chloride of silver formed, which by virtue of my law interfered with current. Cleaned this off two or three times during the expt. Still at last the Electro-chemical results the same as before, for Gas in the Volta Electrometer = 1·42 c: i: and that in the tube *a* ·94 or 2/3rds.

1278. Made a piece of Charcoal the P. Electrode in the Mur. Acid; in this case Carbonic oxide and acid formed.. But no change in the proportions decomposed, for the gas in the Volta Electrometer was = 1·37 c: i: and that in tube *a* 0·9 or 2/3rds.

1279. Used the charcoal P. Electrode also in the first Sulphuric acid. Still results the same, for Gas in Volta Electrometer = 1·68 c: i: and that in tube *a* 1·13 or 2/3rds.

1280. Now used Copper P. Pole in the S. A. It comes between the Zinc and the Platina in its relations. Still no difference in the results, for Volta Electrometer gave 1·76 c: i: of gas and the tube *a* 1·18 c: i: or 2/3rds.

1281. Hence whether any of these bodies or the Platina as in the Volta Electrometer be used, no difference in the quantity Electro chemically decomposed results.

1282. The fuzed chloride of silver agrees with this in another class of decomposing bodies.

1283. Chloride of tin perhaps shew same thing very well.

2 JANY. 1834.

On decomp. of Sulc. Acid.

1284. Well boiled S. A. when cold conducts pretty well. The gas at P. Electrode, much of it dissolved. At N. Electrode Hydrogen and sulphur. The battery weak.

1285. Used a well charged battery—effects as before. The concentrated conducts very much worse than the dilute acid. The oxygen as before much dissolved at P. Electrode. At N. Electrode the hydrogen and sulphur as before. The gas here is not Sul. Hy. but *pure hydrogen*. This proves that water decomposed. I do not believe that the Sul. acid is. Would be well to try anhydrous sul. acid if one had it.

1286. Does not Boracic acid in solution act as S.A. in solution, i.e. increase conducting power but is not itself decomposed?

On decomp. of bisalts.

1287. Put more than enough Oil of Vitriol to Sul. Soda to convert all into bi sulphate of soda, evaporated in crucible until water alone ceased to rise but was accompanied with fumes. Being introduced into the circuit it conducted and decomposed, producing gas *both at P. and at N. Electrodes*.

1288. The heat was then continued and from time to time the fluid submitted to the action of the current. As the heat was applied, the salt required a higher fuzing temperature, and at last fumes of S.A., i.e. oil of vitriol, made their appearance. These continued until and after the salt required a dull red heat, and did not cease before the whole became neutral. It seemed that so long as the acid was in excess, water was retained, i.e. that the bi-salt never became anhydrous.

1289. The substance at all times conducted and was decomposed and *so long* as any bi salt was present gas was evolved at the N. Electrode, and I think it was hydrogen. Hence another proof that water was there, and that as to action of *bi-salts*, the experiment is not decisive. The gradual evaporation was carried on over a spirit lamp.

1290. On law of conduction.

A Crystal of Sulphate of Soda
Sulphate of Magnesia
Carbonate of Soda
Phosphate of Soda

was each in turn put between two plates of platina and subjected to the action of the Voltaic current. The surfaces of the crystals were made large and flat and the plates wetted where they touched them, to make perfect contact in strong solution. The middle

part of each crystal was dry. Some of them were not more than $\frac{1}{8}$ of an inch thick. Yet in all of them there was neither conduction nor decomposition.

1291. This strikingly proves the law as regards water, for instance; for in these salts about and above *one half* of each crystal *is water*—but in the solid state.

3 JANY. 1834.

1292. *Conduction of Phos. acid, etc.*

Pure fuzed Phosphoric acid heated in a crucible or on a ring of plat. wire conducts and is decomposed. But it is water which is decomposed, not the acid.

1293. In the first place, the acid will evaporate entirely, forming fumes, so that one cannot obtain a fixed acid as with boracic acid. In the next, there is always gas at *both poles*, and the gas at the N. Electrode is not Phosphuretted hydrogen, for it does not burn phosphorically and there is no smell of Phosphorus during the whole time. Hence it would appear also that the P. Acid does not give way in a secondary manner but is volatilized. It is true that the Phosphorus evolved either way might be burnt before any smell could be evolved from it, but the appearances seem otherways, and to account for the gas at the N. Electrode there must have been water in the acid. Again, the N. wire is not acted on or dissolved, as happens with some bisalts to be described.

Bi-salt, Bi-phos. soda.

1294. Made a bi phosphate of Soda by fuzing the ammonic phosphate of Soda, and then acted on that heated on a ring. This salt continued to boil slowly at a high heat per see, and when tranquil and clear by a full red heat, a white heat could make it bubble. Hence it is doubtful whether it was quite freed from water.

1295. When calm and quiet at a white heat, it did conduct and decompose. A phosphuret of platina seemed to be formed at the N. Electrode. This looks very much like a true decomposition of a dry bisalt, but am not quite sure, for Phos. hydrogen seemed to be evolved sometimes (perhaps from phosphuret of sodium however). The salt at last also was nearly neutral. If this be a true decomposition it is possible or unlikely that it may be con-

sidered as a solution of neutral phosphate of Soda in Phosphoric acid.

1296. Made a Biborate of Soda by fuzing together 2 parts of Borax crystals and 1 part of Boracic acid crystals. This is rather more than the equivalent of B. A.

1297. This salt I think when ignited must be dry. It however decomposed and conducted very well. There was gas at *both Electrodes* and much alloying of the platina and consequent fusion at the N. Electrode, probably from reduction both of Sodium and Boron.

1298. More Boracic acid was added: still the effect was the same, but more slowly—gas yet at the N. Electrode?

1299. Can these again be considered as solution of Borax in Boracic acid? Perhaps they may even be rightly considered as the *base only* in solution in B. A., and the same with the sulphate and Phosphate.

1300. The Boracic acid alone did not conduct in the least.

1301. When a dry salt decomposes, the arrangement of substances between the Electrodes must become very complicated.

P.	P.	P.
Platina ———	then Plat. ———	then Plat. ———
		Oxygen
Sul. of Soda	S. A.	Sulphur $\downarrow$
	Sul. Soda	Sul. Soda
	Soda	Oxygen $\uparrow$
		Sodium
Platina ———	Plat. ———	Plat. ———
N.	N.	N.

Examine all this, for how can conduction go on with S. A. or P. A. or B. A. against the P. Electrode, as in the second step?

4 JANY. 1834.

1302. Water as dilute S. A. was decomposed in two volta-electrometers, one with large plates, the other with small wires, using a battery of only 9 prs. of plates. The quantity of Oxy. and Hy. evolved was alike in both, i.e. 52 volumes.

1303. Stirring the acid with a feather between the plates of a battery very much increases its power; in a weak battery, stirring

caused the evolution of bubbles from the beak of the retort volta electrometer twice as fast as without.

1304. Pure water and a pure solution of Boracic acid were tried as to conducting power against each other; the last conducted 20 or 30 times better than the former.

1305. S. A. s.g. 1·270 and saturated solution of Sulphate of Soda were tried against each other as volta electrometer fluids—relative to absorbability of gases, as oxygen, by them. They were arranged thus*:

The battery was weak in action.

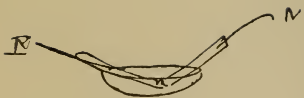
1306. After the expt. was over, the gases were measured by two different measures.

			by one measure		by another measure.
Tube 1.	Pos. in Sul. Soda	gave	5·4 vols.	and	25·9
2.	Neg. in Do.	„	12·4 „	and	63·1
3.	Pos. in dil. S. A.	„	6·2 „	and	31·8
4.	Neg. in Do.	„	12·4 „	and	63·3

The dilute S. A. therefore much more exact than the Sul. Soda.

1307. It seems here as if in tube 1 absorbtion had happened before it was measured the first time, and also between that and its second measuring. Therefore repeated the decomposition with the Sulphate of Soda solution only. Tube 1, Positive, in the Sul. Soda gave at first 18·2 vols. of gas; by standing, with a little agitation, this diminished to 16·9, and I believe the effect was real, not an accident. It still *smelt strongly* of *Electricity*.

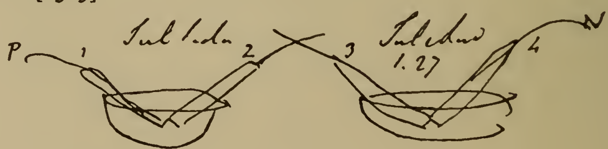
1308. The contents of tube 2 I lost, and I must defer further examination to save time just now with Series 7 and 8.



1309. Ascertained proportion of oxygen at P. Electrode from Muriatic acid, 1 vol. of Strong solution diluted with 99 vols. of dilute Sul. Acid. After the expt. the N. tube contained 12·8 vols. of Hydrogen. The P. tube contained only 3·4 vols. of oxygen, which had belonged to 6·8 of the hydrogen; the other 6 of hydrogen must have been from Muriatic acid and must have given 6 vols. of oxygen, but they dissolved in the liquid. Very nearly equivalents of M. A. and water decomposed in this case.

1310. Made an expt. on intensity with large and small decomposing plates. Both jars contained the same very dilute S. A. In

* [1305]



the large jar large platina plates were used; in the small jar small platina wires*. On bringing the ends of a volta electrometer against *a* and *b*, very little decomposing action occurred because the current had very little intensity there. But when brought against *c* and *d* great decomposing action occurred, because of the great intensity there. Hence intensity has nothing to do with the amount of chemical action, since that is the same in the two jars; it all depends upon the quantity passed.

1311. Tried to collect chlorine from Strong M. A. at P. Electrode, but the solution absorbed it all.

1312. Put about 1 vol. sol. M. A. to 4 vols. strong oil of vitriol, to try and prevent solvent power on chlorine. But still not good result. The gas at the P. Electrode was about $\frac{2}{3}$ mur. acid, $\frac{1}{6}$ chlorine, $\frac{1}{6}$ oxygen.

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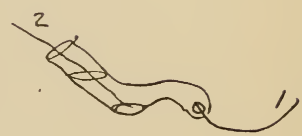
1313. Experimented on transfer in fused bodies, and first on chloride of lead. Wire 1 was as usual fixed in the closed end of a tube, but the tube so bent that the melted metal at the end of wire 2 should not mingle with that collected in the expt. at wire 1. Wire 1 had the knob of platina at the end and weighed at first 21 grains. Wire 2 had a similar knob, but after that a button of lead was alloyed with and fuzed on to it in a little glass tube. It weighed 29·3 grains altogether.



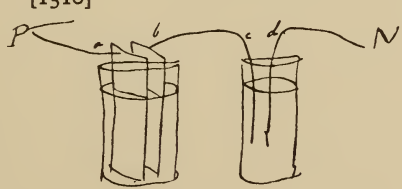
1314. During the experiment all was placed as in the figure, and the chloride retained fluid by two spirit lamps, and thus the voltaic action was continued. No gas was evolved at either 2 or 1, for as fast as lead was reduced at the end of 1 it was dissolved by the chlorine at the end of 2.

1315. At the end of the experiment 3·78 c: i: of gas had been collected, = 0·488376 of grain of water decomposed.

1316. On opening the tube the buttons of alloy on both 1 and 2 wires looked very well.

		gr.	
1.	Wire of platina now weighed	26·4;	it was 21; gain 5·4.
2.	Wire of platina and lead ,,	24·2;	it was 29·3; loss 5·1.

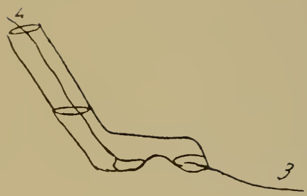
* [1310]



1317. Hence for a first expt. the gain and loss tell very well together.

$$\begin{array}{l} \qquad\qquad\qquad\quad \text{W} \quad \text{L} \\ \text{Now } 0{\cdot}488376 : 5{\cdot}4 :: 9 : 99{\cdot}5 \\ \text{and } \;\; 0{\cdot}488376 : 5{\cdot}1 :: 9 : 94 \end{array}$$

1318. Here both too small, but if the gas had been only 0·15 of cubical inches less, it would have been right, i.e. if it had been 3·63 c: i: instead of 3·78 c: i:; must examine the cubic inch measure, and also note Bar. and Therm., and also measure again next day. Now the correction for moisture only, taking the gas at 60° F, amounts to 0·06, which removes $\frac{2}{5}$ of the deficiency.



1319. Repeated this expt., but had a better tube, so that a little bubble of gas from wire 3 could get away. During the expt. a very little gas issued from the end of wire 3, but altogether did not make a bubble larger than this ⬭ when dull red hot. Whether due to a minute portion of moisture retained in the tube, or as is most likely, to the air previously involved in the folds of the plat. foil round that end of the wire, is not quite sure. I think it was the latter. No gas came from the end of wire 4 during the whole expt.

Wire 3 weighed at first 20·55 gr.; weighed after 27·75; gain 7·2.
Wire 4 „ „ 43·3 gr.; „ „ 36·3; loss 7·0.

The quantity of gas evolved was 5·12 cubic inches, which, corrected for moisture only = 5·025 = 0·64923 of gr. water decomposed.

$$\begin{array}{l} \qquad\qquad\qquad \text{W} \quad \text{L} \\ \text{Now } 0{\cdot}64923 : 7{\cdot}2 :: 9 : 99{\cdot}81 \\ \text{and } \;\; 0{\cdot}64923 : 7{\cdot}0 :: 9 : 97{\cdot}04 \end{array}$$

1320. In this case I found the P. Wire button very cavernous; at first it weighed 37·6, but on breaking it open and boiling it in water to remove the evident chloride of lead, it was reduced to 36·3 gr.

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1321. Now experimented with *Iodide of lead* in the same way.

The affair went on very well. Bar. 29·7, Therm. 54°. The quantity of gas = 5·45 c. inches in Volta electrometer.

The N. Electrode weighed at first 19·3; it became 26·7; gain 7·4.
The P. Electrode and appended lead 55·35; it became 47·35; loss 8·0.

Now 5·45 c:i:, corrected for moisture, are equal to 5·37 c:i:, and these are = 0·6938 of gr. of water.

W L
Then 0·6938 : 7·4 : : 9 : 96
and 0·6938 : 8 : : 9 : 103·7

There was a little confusion in this expt. as to the N. Wire with the one meant for another expt. But the form good. No iodine appeared the whole time—all went on well.

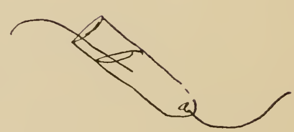
1322. *Chloride of tin*, Proto, with Tin at the P. Electrode as in the cases just described. All went on beautifully. *No bi-chloride* was formed, but there was a little gas, I think hydrogen, from the N. Electrode at the commencement of the experiment. It was from water that could not be dissipated even at a dull red heat, but the quantity was very small; no proportion to that in the Volta Electrometer.

The gas in Volta Electrometer = 5·65 c:i:, which corrected for moisture = 5·56 c:i: = 0·718352 of gr. water.

The N. Electrode, Plat., weighed at first 17·3; after 22·05; gain 4·75.
The P. Electrode, Plat. and Tin „ „ 48·43; „ 43·7; loss 4·75[1].

W T
Now 0·718352 : 4·75 : : 9 : 59·51

This rather too high, but the gas has not been corrected to temp. and Pressure—besides which the smallest error in measurement makes the difference. Good general expt.



1323. Now examined per iodide of mercury as to its power of conducting, decomposing, etc. This form of expt., and both Electrodes platina. It certainly does not conduct when solid and does conduct when melted, and the current decomposed the water in the Volta Electrometer, although very slowly compared to former cases, as of iodide lead, chloride, etc. Hence statement in account of law of liquido-conduction *true*.

1324. As to consequent decomposition, I could see none. There was no appearance of iodine at the P. Electrode all the time, nor any appearance of a new body formed. On after examination the P. Electrode wire was not found affected and the N. Electrode wire was also [?not] changed; no appearance of mercury or any other metal there.

[1] Query 4·73.

1325. The case may be one of conduction without decomposition, being then however the only one I know of amongst bodies subject to the law of liquido-conduction; or the effect may be due to the presence of a little protiodide in solution in the per iodide, and as water is retained by chloride of tin that is not at all unlikely. Then such protiodide would carry on the effect, and yet from formation of per iodide at P. Electrode, and as mercury deposited at N. Electrode, formation of protiodide there, no accumulation of any thing at either cisode or exode would appear. The secondary results in fact merging in the bodies already present.
1326. The case is only important as to the principle of action, and being alone cannot disturb any general law.
1327. Tried to obtain *Electrochemical equivalents with secondary results*, as by precipitating the metals.
1328. *Solution of Acetate of lead* or any solution of lead will not do. There is no gas at N. Electrode, but the crystals of lead shoot across into the solution and communicate with the P. Electrode.
1329. Solution of *Proto nitrate of mercury*, using a weighed platina wire as N. Electrode. Here gas was evolved from the N. Electrode, although only in small quantity, but that confused the result. The gas evolved was 1·96 c. inches; corrected for moisture = 1·93 c: i: = 0·249356 of gr. water decomposed. The wire weighed at first 21 gr.; became 24·6; hence mercury reduced 3·6.

$$\text{Now } 0{\cdot}249356 : 3{\cdot}6 :: \overset{\text{W}}{9} : \overset{\text{M}}{130}$$

This too little. I fear that a per oxide is formed at the P. Electrode and that the hydrogen at the N. Electrode is partly lost on reducing this. The formation of gas also tends to make the quantity of mercury too small.
1330. Repeated the expt. (in the same portion of solution of proto nitrate) using a piece of Plat. foil instead of platina wire. Gas evolved 2·2 c: i: = 0·28424 of gr. water. Platina foil at first 17 gr. became 21·25; increase 4·25 mercury.

$$\text{Now } 0{\cdot}28424 : 4{\cdot}25 :: \overset{\text{W}}{9} : \overset{\text{M}}{134{\cdot}5}$$

This is better, and there was less gas at the N. Electrode, but probably the prop. of per nitrate is increasing.

1331. In both these expts. a reddish deposit occurred on the P. Electrode. Can it be per oxide of lead or the matter from Zinc solution, etc. etc.?

1332. Repeated the expt., putting first some acetic acid into the same portion of Proto nit. mercury to prevent if possible the formation of per oxide there. The gas evolved in Electrometer was 2·62 c: i:; corrected for water = 2·58 c: i: = 0·33336 of gr. water. The mercury reduced was 4·75 gr.

$$\text{Now } 0{\cdot}33336 : 4{\cdot}75 :: \overset{\text{W}}{9} : \overset{\text{M}}{128{\cdot}2}$$

This less again; probably due to the per oxide formed in former actions.

1334[1]. Must try some day a solution of Proto Acetate of mercury in Acetic acid.

1335. Now experimented *on Sul. Zinc*, using Plat. foil as the Neg. electrode, in a strong solution.

The quantity of gas was 1·04 c: i: = 1·024 c: i: = 0·1323 of gr. The Zinc deposited was 0·5 of grain.

$$\text{Now } 0{\cdot}1323 : 0{\cdot}5 :: \overset{\text{W}}{9} : \overset{\text{Z}}{34{\cdot}08}$$

As 32·5 or 33 is considered the equivalent of Zinc, it shews a very close approximation, especially in such small quantities. This will probably do for galvanometric indication.

1336. There were two things to observe and pursue here.

1337. In first place the zinc crystallized on the Plat. foil, but the crystals did not project from it as those of lead or silver would do in their solutions, but were applied close and flat to the Platina, investing it thoroughly and not running off into the solution. This extension of them *across* the course of the Electric current instead of *along* it curious and worth pursuing. Will probably indicate the relative strength of the aggregation in one direction and the other *relative to the direction of the natural electricity belonging to the particles of Zinc, silver, lead, etc. etc. etc.*

1338. Then in this solution a red colour and deposit formed at the *P. Electrode.* Probably the same as I observed before and marked down, only now it separated from the plate and was diffuzed thrgh. the fluid. It also has a curious almond, or prussic,

[1] 1333 has been omitted in the MS.

or odoriferous smell of some kind. Cannot help thinking there is a new substance here. I wish I could evolve *time* from the battery as well as all these points; *and then—See 6th Feb.* 1834.

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1339. The condensed Nitrous or Per nitrous acid from Nit. lead. It has been in contact with Sul. Acid—was poured from off it—was yellow—and poured rather thickly.

1340. It conducted and decomposed. There was gas at both poles but that at the N. Electrode seemed to be immediately recondensed. Examined the gas at the P. Electrode; it had every appearance of oxygen. Examined that at the N. Electrode. It was reabsorbed before the bubbles could rise an inch in the liquid. It looked like nitrous acid gas or perhaps nitrous gas. I believe water was present and that the results were secondary.

1341. On putting a drop of water to the crystals remaining, gas (N. Gas) was evolved and a deep green solution formed on the top of the remaining crystals, but on shaking all together more of the crystals disappeared, more liquid formed and that liquid was yellow; yet though it contained water it was just like that I had used. Hence the first might have contained water.

1342. A little water added to this yellow liquid evolved Nitrous gas and made the liquid deep green, and that agitated with the remaining crystals again became yellow.

1343. *Proto chloride of Carbon*—will not conduct or decompose.

1344. *Hydro chloride of carbon*—will not conduct or decompose.

1345. *Chloride of Sulphur*—Do.

1346. *Proto chloride of Phosphorus*—Do.

1347. *Arsenic acid*—doubtful. Can not get it dry. What I did get conducted and decomposed, but plenty of water there.

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1348. *Electricity of a single circle* and its equivalent of matter decomposed.

1349. Two pieces of sheet zinc amalgamated and weighed; the one weighed 165·1 gr.; the other, No. 2, 147·4 gr.

1350. Some dilute Sul. Acid was made so as to act quickly on zinc, but not on amalgamated zinc. This was put into an earthen

ware trough and a jar filled and inverted in it. A platina spatula was put into the jar in the acid. The larger piece of amalgamated zinc tied to a stick, put into the jar and held against the plate of platina so as to form a voltaic circuit, whilst the other piece of zinc was put into the same acid but out of contact with the platina.

1351. The platina immediately evolved gas in abundance, but none came from the zinc touching it. After a while the experiment was stopped and the zinc put into water to be washed. The piece No. 1 had in the mean time had just one coat of small bubbles formed on its surface, but scarcely one had escaped from it; the quantity insensible.

1352. The two pieces were now again dried and weighed.
No. 1 weighed 147·35; it had lost only 0·05 of a grain.
No. 2 weighed 158·8 ; it had lost 6·30 of a grain.

The volume of gas was 10·02 cubic inches, and was hydrogen. The thermometer was at 54°, the Barometer at 29·2 only; the correction for Barom., therm. and water brg. the gas to 9·695 c: i: at mean temp. and pressure. This is the hydrogen: to which add 4·847 for the oxygen and it makes 14·542 c: i: of oxy. and hy. from water decomposed *voltaically*. Now these weigh 1·878826 gr.

w z
and 1·878826 : 6·3 : : 9 : 30·2 nearly.

This very near for a first expt. Number of Zinc is given as 32·5.

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1353. Repeated the above expt.—used the same acid, but had put a small piece of zinc in last night to dissolve, and by its minute bubbles to clear away all air tending to separate from the solution. The same pieces of zinc wire also used, but amalgamated a little more.

Small piece weighed at first 148·3; being left in the acid during the expt. and weighed again at the close, it was still 148·3. The bubbles formed on it during the time were scarcely perceptible.

1354. The large piece of zinc weighed at first 163·1 gr. and after the expt. 154·65. The zinc dissolved therefore amounted to 8·45 gr.

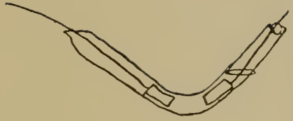
1355. The gas collected was 12·5 cubic inches, Temp. 52°, Bar. 29·2 inches, which corrected for temp., pressure and moisture becomes 12·15453 cubic inches of dry hydrogen at temp. 60° and Pressure 30 inches. This is the Hydrogen, and increased by one half for the oxygen, it becomes 18·232 c: i: nearly = 2·3535544 gr. of water decomposed voltaically.

$$\text{Now } 2{\cdot}3535544 : 8{\cdot}45 :: \overset{w}{9} : \overset{z}{32{\cdot}31}$$

Excellent.

1356. *Requisite Electricity for decomposition of water* 1 *grain.* A wire 2 inches in length in the clear part and $\frac{1}{104}$ of an inch in diameter, Platina, was introduced into the circuit with the Volta electrometer, and a jar marked at the part = 8·4 c: i: of gas, for this quantity is when corrected for moisture, etc. etc., Bar. 29·2, Therm. 54°, as nearly as may be the results of 1 grain of water decomposed. Adjusted the battery so that it should retain this wire pretty steadily red hot, i.e. sensibly so in ordinary day light. It required this current to be continued for 3·75 minutes to produce the 8·4 c: i: of gas.

1357. On repeating the expt. the same result obtained. Hence a current equal to the heating of this wire red hot must be continued $3\frac{3}{4}$ minutes before enough electricity has passed to equal that in a grain of water.



1358. *Decomposition of some solutions* for secondary result section. Used the following tube apparatus in most of these expts., indeed in all, turning it round to obtain the products at the second pole, so that with each expt. on a substance there were two parts of the expt.

1359. *Hydriodic acid.* The solution made by adding sulphuric acid diluted with 4 or 5 times its weight of water to a pretty strong solution of Iodide of Potassium. There was good action. Much gas at N. Electrode—little gas at P. Electrode, but much iodine there being deposited even in the solid form, but soon dissolving in the Hyd. acid when Electric current ceased.

$$\left\{\begin{array}{ll}\text{At N. Electrode gas Hydrogen} & = 0{\cdot}60 \text{ of cubic inch} \quad 2\\ \text{At Volta Electrometer both gases} & = 0{\cdot}87 \quad ,, \quad ,, \quad 3\end{array}\right\}$$

At P. Electrode gas Oxygen	= 0·08 of c: i: only	This as with Mur. acid in dilute solution and for same cause.
At Volta Electrometer both gases	= 1·12 „ „	

1360. Repeated the expt. as to collection of hydrogen at N. Electrode.

At N. Electrode gas hydrogen = 0·56 c: i: – 1·82
At Electrometer = 0·92 c: i: – 3

1361. This deficiency of hydrogen is due I believe to the expt. being made with the same solution, in consequence of which the free Iodine liberated in the first expt., and remaining dissolved in the liquor, combined with a part of the nascent hydrogen and thus lessened that evolved.

1362. Sol. *Iodide of Potassium.*

At the Pos. Electrode: No gas, only Iodine, solid, but dissolving by degrees. At N. Electrode: Hydrogen and probably free alkali formed. The gas = 0·7 c: i:, whilst that from volta-electrometer was 1·05 c: i:, the two being exactly as 2 : 3, as they ought to be.

1363. *Cyanide of Potassium*, dissolved in water at the moment smells prussic and is alkaline to turmeric, but that a character of the cyanide. The results were very good. There was *no gas* at the P. Electrode, but a brown solution formed there as if cyanogen separated at the cisode, but redissolved by the water and dissolved cyanide. At the Negative Electrode hydrogen only was evolved, the quantity being 0·6 of c: i:, whilst the gas in the Volta Electrometer was very nearly 0·9 c: i: or as 2 : 3.

This accords with chloride and Iodide of Potassium.

1364. Added Sul. Acid to this as before to Iodide and then experimented as for *Hydrocyanic acid.* Now Gas at both poles and no discoloration at the P. Electrode.

At N. Electrode hydrogen = 0·6 c: i:, but mixed with prussic vapour as appeared by its burning.
Volta Electrometer = 0·78 c: i:; repeat this.

At P. Electrode the gas diminished a little by agitation with water and was therefore either carb. acid or perhaps cyanogen, and left only 0·35 c: i: of *oxygen*, the gas in Volta Electrometer

being = 2·37. Hence probably both water and Hydrocyanic acid decomposed here as in case of muriatic or hydriodic acid, and both oxygen and cyanogen set free, but the latter dissolving. Whole expt. worth repeating both as to evolution of free cyanogen at P. Electrode, and any thing with the Hydrogen at the N. Electrode.

1365. Must try fuzed cyanide of Pm. for cyanogen at the P. Electrode—will be a curious case of the passing of nitrogen and Carbon to the *P. Electrode.*

1366. *Solution of Cyanide of Mercury*—does not conduct a sensible quantity. Added Sul. acid, then at N. Electrode some gas, hydrogen, and some mercury. The proportion of gas increased as battery power increased or as strength of solution diminished. At P. Electrode gas oxygen, but only 0·22 of cubic inch, whilst at Volta Electrometer 1·08 c: i: The action here probably as with Sol. Hydrocyanic acid, with a secondary result at the N. Electrode.

1367. Added sol. sul. soda to sol. Cyanide of mercury; still gas was evolved at the N. Electrode.

1368. Solution of *yellow ferroprussiate of potassa*. Gas at both poles but very little at P. Electrode.

N. Electrode, hydrogen = 0·68 c: i: } nearly as 2
Volta Electrom. = 0·97 c: i: } 3

At P. Electrode a little prussian blue; gas oxygen = 0·14 c: i:, whilst in volta Electrometer it was 1·55 c: i:. Hence ferro cyanogen principally at P. Electrode.

1369. *Acetic acid*, diluted with dilute Sul. Acid. Gas at both poles.

{N. Electrode, hydrogen = 0·63 c: i: } 2 nearly
{Volta Electrometer = 0·93 c: i: } 3
Volta Electrometer = 2·27 c: i:
P. Electrode = 0·55 c: i:, which by agitation gave up Carb. acid and diminished to 0·44, which were oxygen.

1370. *Strong solution of Tartaric acid*, almost a non conductor—if quite pure, probably quite like water or acetic acid or ammonia or Cyanide mercury, etc. etc.

Put in some Sul. Acid, then powerful action and gas at both poles.

Volta Electrometer 1·0 c: i:
N. Electrode 0·64 c: i:; this was hydrogen.
Volta Electrometer 1·7 c: i:
P. Electrode 0·55; very little absorption here; nearly all oxygen. Hence here but little secondary action. But with such solutions the results differ much with different strengths of current and also different tubes, as when poles are thus*, or thus*, etc. etc.

1371. *Solution of Tartrate of Soda* in tube—action and gas at both Electrodes, but assumed that that at N. Electrode would be hydrogen, and examined only that at P. Electrode.

Volta Electrometer 0·59 c: i:

P. Electrode gas 0·5 c: i:; this a large quantity. Gas not absorbable by water or potash. On putting light into it it appeared very like common air. There was oxygen present diluted with some other gas produced in rather large proportion, and this was not carbonic oxide, carbonic acid or any combustible gas; ? must examine this.

1372. *Solution of Benzoate of Soda* with excess of soda to make it soluble. Is a bad conductor, but gas appears at both poles. In some of these cases, where the gas is frothy, the badness of conduction may be due to the attenuation of the liquid by the mixture of air or gas.

Volta Electrometer 0·36 c: i:

P. Electrode 0·10 c: i: Principally oxygen, but quantity too small to examine and compare with certainty.

1373. *Syrup of sugar* mixed with Sul. acid; 1·30 s.g. Plenty of action at both poles; only examined that at P. Electrode. A little of it was absorbable, the rest brightened a coal but exploded by flame—was a mixture of oxygen and carb. oxide which with Carb. acid had been evolved.

The unabsorbed gases = 0·4 c: i: } hence much secondary
Volta Electrometer ,, = 2·3 c: i: } action.

1374. *Solution of Gum arabic*, with Sul. acid s.g. 1·30. Plenty of action.

Volta Electrometer = 1·8 c: i:

P. Electrode only = 0·35 c: i:; after absorption there was

* [1370]  

some oxygen present but also some other gas, perhaps carbonic oxide.

1375. *Solution of Albumen in Potash.*

A very bad conductor. Gas at both poles, frothy very.

Volta Electrometer 0·62 c: i:

P. Electrode 0·08 c: i:; this oxygen. This gas had to stand so long to clear from froth that any carbonic acid would have been absorbed.

1376. I do not think that alkali favours conducting power of water much, *if at all.* Must try it quite pure and free from Carbonic acid and other acids.

1377. *Solution of Rosin in Potash*: to this added solution of sulphate of soda to make it a conductor. Conducted then very well. Gas at both poles; only that at P. P. examined.

Volta Electrometer 1·84 c: i:

P. Electrode about 0·42 c: i:; was oxygen nearly pure, but had been standing some time over water to clear off bubbles and froth.

1378. Now Experimented with *Fluoride of Potassium* in solution. There was gas at both Poles, but no sensible action during the whole time on the glass.

Volta Electrometer 0·84 c: i:

N. Electrode 0·55 c: i:; this hydrogen, and to the gas in Volta Electrometer as 2 to 3. Good.

Volta Electrometer 1·86 c: i:

P. Electrode only 0·47 c: i:, and this was oxygen. There was no action on the Platina.

1379. Then added Sul. acid to the same solution. There was immediate and powerful action on the glass, effected of course by the hydro-fluoric acid now in a free state. Subjected this to the battery action as a solution of *Hydro-fluoric* acid; there was *much gas at the N. Electrode*, but *little at the P.*

{Volta Electrometer 0·99 c: i:
{N. Electrode 0·67 c: i:; this hydrogen, and just as 2 : 3.

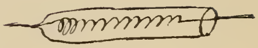
Volta Electrometer 1·81 c: i:

P. Electrode only 0·20 c: i:, which was oxygen. Still the Platina Electrode was not apparently acted upon and the glass in the neighbourhood was. From which, and from the small quantity of oxygen, I conclude that Fluorine had been evolved

at the Electrode and dissolved in the water *without decomposing it*, but something in the manner of chlorine or iodine. If not so, then the Platina ought to have been corroded or else the full equivalent of oxygen set free.

1380. If an electric current can heat one inch of wire it ought to heat 100 inches, provided the same quantity passes in the same time; i.e. if the larger length is not heated as much as the smaller, it must be because *less* is passing. For its having heated one portion is no reason why it should not heat another. There can be no other proper known reason.

1381. Contrive mode of heating wires—



17 JANY. 1834.

1382. An expt. was begun on the 10th instant on comparison of Large and small voltaic battery. Two wires, one of platina, the other of zinc, each $\frac{1}{18}$ of inch in diameter and 5 inches long, were fastnd. together at ends by small cross binding pieces of platina wire so as to keep the ends about $\frac{3}{8}$ of inch apart, but crossing and touching in the middle.

1383. A dilute S.A. consisting of 120 oz. Water distilled and 30 drops strong oil of vitriol was put into a large jar and the above arrangement suspended in the middle of it by a thread. It acts fairly; gas appears on the platina; it looks quite as strong if not stronger than the little test battery. This was put into action at 25′ to 11 o'clk. A.M., 10 July [? January]; the zinc wire weighing at that time 17·5.

1384. On the 11th instant at Evening the platina had become dull from precipitation on it and the gas bubbles were present on the zinc; hence not acting as a voltaic circuit—the two metals now equal and probably act as Mariani [? Marianini] describes. On cleaning the surface of the platina by rubbing with the fingers then acts there and evolves gas. Added 30 drops more of strong Oil Vitriol to the fluid to increase effect and compensate for that neutralized by the zinc dissolved.

1385. On this day the 17th, about 10 o'clk., and therefore very nearly a week after the commencement of the experiment, took out the wires, separated them and weighed the Zinc. It was equal to 10·7 grains and had lost therefore 6·8 grains.

1386. The solvent action was going on very well on the zinc, but no gas on the platina; but all things considered, I think the zinc has been dissolving as fast as that was doing in the little battery used for comparison in the third series of Researches.

1387. Now in this form of battery 1 gr. of water require solution of 3·6 grains of zinc—and as 6·8 gr. dissolved in 7 days, 3·6 would require 3·7 days; but if a wire 5 inches long required 3·7 days to loose 3·6 grs., one only $\frac{5}{8}$ of an inch in length but of the same diameter would require 29·6 days for solution of same weight, if constant action could be sustained. Now the comparitive battery required 0·0533 of a minute to equal one charge of Leyden battery, but 29·6 days divided by 0·0533 of a minute gives very nearly 800,000. So that from this calculation the electricity required to decompose a single grain of water is about equal to that of 800,000 charges of the Leyden battery, any one of which would kill a cat or dog.

1388. The zinc was returned, weighing 10·7 grains, into the acid at 10 A.M. Friday the 17th Jany., having first had a fine platina wire coiled spirally round it. This was to see that plenty of acid remaining. Action very fair.

1389. Prot. iodide of tin: fusible by high heat in close tube but in open air burns by heat with bright flame, per iodide subliming and prot or per oxide remaining. Not a good substance for Prot. and per experiment—requires too high a heat to fuze it.

18 JANY. 1834.

1390. The zinc wire (put in acid on the 17th, i.e. yesterday), taken out to-day at $\frac{1}{4}$ p. 3 o'clk., having been in the acid $29\frac{1}{4}$ hours. It weighed 10·1 grains, having lost 0·6 of grain. The mean rate of the solution during the 7 days is about twice as fast as this rate. Now considering the extent of the present rate and the double strength of acid given at the commencement of the expt., I think the former rate must at any one time have been as fast, or great rather, as the rate upon the comparitive voltaic couple, and there-

fore that the proportion of 800,000 then deduced is certainly beneath the mark; the probability is that 1,200,000 is nearer the truth.

1391. Fuzed acetate of Soda does decompose; it is an Electrolyte, but there is much secondary action both at the cisode and exode.

1392. *Fluorine, etc.* Made a sol. of Fluoride Potassium, added a little S. A. to evolve Hydro-fluoric acid. Put this into a platina crucible rendered Positive and dipped a plat. spatula rendered Neg. into it; action as on former occasion (very different to that occurring if the neutral solution of Fluoride be used). After a minute or two, separated the Voltaic battery and added a little solution of Sul. Indigo to the liquor. *It bleached.* Hence additional proof of the production of an aqueous solution of fluorine—of its inaction on platina (for the crucible was not affected)—also a proof in its bleaching power of analogy with chlorine. The solution smelt strong and acrid but not like chlorine.

1393. I begin to think that fluorine may probably be received and retained in glass vessel though *Hydro-fluoric* may not. It does not follow that cold fluorine should decompose an oxide because cold the fluoric acid does. For chlorine does not decompose lime at common temperatures though Muriatic acid does.

1394. May also use vessels of wood, paper, oiled paper, oiled or waxed silk or cotton, etc. etc. etc. strengthened by wood outside, to receive, to retain and even to evolve fluorine. Apparently also use platina vessels well at low temperature. There is less action on it by Fluorine than by chlorine.

VOLTAIC CURRENT—ITS INTENSITY AND EVOLUTION.

1395. Tried (Daniell present) to send current of a *single voltaic circle* through my Volta electrometer. Used amalgamated zinc as the motor with platina. Thus two cups, 1 and 2, with pretty powerful S. A., the same as in the volta electrometer 3. All the metallic communication wire platina, 4, except one piece 5, which was zinc. But in this state of things there was no current. The reason is good, but *very important to me.* To produce a current water would have to be decomposed in cup 1, but to transmit a current water must be decomposed both in cup 2 and in volta electrometer. The force to be overcome was twice that of the originating force.

1396. When cup 2 was dismissed and the zinc 5 communicated directly as by the dotted line with the Volta electrometer, still no decomposition occurred, for the producing and the opposing decompositions are now equal.

1397. It seems most likely that strength of acid has nothing to do with the occurrence or non-occurrence of these counter decompositions, but that the number of decompositions which on the one hand excite or produce the current and on the other retard it constitute the essential point. Then 10 platina diaphragms used to decompose would neutralize the current of a battery of ten pairs of plates, etc. etc., no respect being had to the strength of the acid, etc. etc. either in one or the other sets of cells. *Beautiful. I think I see it all*, but must go on with *fluorine first.*

20 JANY. 1834.

1398. To obtain electricity by the evaporation of water, it must not be pure but a solution—(query chemical action then?). So at least Arago says: Arago's Eloge, p. 23. Harris says not.

1399. When on origin of Electricity in Voltaic pile and even on other Electrical points, refer to Arago's Eloge on Volta, 1833.

1400. *For Fluorine.* Fluor Spar and Ox. Manganese well powdered and mixed with Oil Vitriol in a platina crucible, both without and with heat. Apparently only hydrofluoric acid evolved. The fumes very acid but *did not bleach* litmus paper—only reddened it. No action, or scarcely any, on the platina.

Fluate Potassa, Ox. Mang. and Oil Vitriol in Platina crucible did not succeed better. There was here a little degree of action on the platina, but not much; had also used a higher heat.

1401. Fluate potassa, Red lead and Oil Vitriol— in same platina vessel: the fumes have a smell different to Hydro fluoric acid. More as if a little Euchlorine with it. Are white and fuming; act on litmus, reddening as before, but also a bleaching effect in addition, though very slight. Looks as if a little fluorine set free.

1402. Red lead and Oil Vitriol alone in same vessel. Did not shew the same symptoms of bleaching, but gave same Euchlorine kind of smell; perhaps the smell of the oxygen.

1403. *Fluoride Lead*, precipitated from Fluate Potassa and Nitrate

of Lead, well washed and dried: white insoluble powder. When heated on platina, fuzes very well on fair red heat without injuring the platina or undergoing change. But it gives off acid fumes during the first application of heat and then seems permanent. The fumes redden but do not bleach. I thought at first they were Fluorine and *must be sure they are not.*

1404. When fuzed in Glass tube, the same acid fumes arise. The glass not affected apparently in the part of the tube above by the fumes, but there is a white opaque part just over the fuzed fluoride where red hot. As the melted fluoride solidified and cooled the glass beneath it broke all to pieces. For in fact action had taken place there and the glass and the fluoride were one piece at that surface. The action is not rapid and glass would contain fuzed fluoride some time at red heat. It seems to be a direct union of the fluoride and the glass and not to be accompanied by any evolution of oxygen or other gas. The same effect takes place with the fluoride of Pm. fuzed in glass. It does not happen, nor any thing like it, in Platina.

1405. Fluoride of lead fuzed on Plat. foil, Neg., touched by a Pos. Plat. wire. Much good action at N. foil; lead reduced and Platina alloyed and fuzed. At P. Electrode effervescence much; vapours transparent, pungent, etc. The platina wire acted upon but only a little, not as if a chloride had been acted upon. Is it possible that a *bi-fluoride* should be formed here? Not very likely.

1406. Fluoride of lead fuzed or Neg. Plat. foil, touched by Pos. plumbago. Action as before; much gas or vapour at Plumbago electrode. Very pungent, etc. *No action* on the plumbago itself. The fumes which rise appear to corrode glass, but this does not decide that at common temperatures fluorine would act on glass.

1407. Fluoride of lead fuzed on neg. plat. foil and touched by a plumbago pos. electrode, inclosed in a glass tube as in a chimney for the purpose of carrying the fumes up. Litmus paper held at the top was reddened and I thought a little bleached; but not quite sure.

1408. Fluoride of lead fuzed on glass touched by two plat. wires, P. and N. It is an excellent conductor and a very thin film carried on the action. At N. Pole no gas; only lead reduced. At P. Electrode plenty of gas or vapour and scarcely sensible action

on the platina. Action on the glass every where in contact with the fluoride. Here then the fluorine, gaseous, colourless, pungent, etc. etc.

1409. Neutral Solution of Fluoride Pm. does not act on glass at common temperatures, but any acid makes it act or any acid salt, as Nitrates of Mercury, silver, etc. etc. etc.

22 JANY. 1834.

1410. Precipitated proto nitrate of mercury by solution of Fluoride Pm. If not strong scarcely any precipitate. When strong much grey curdy precipitate, but at same time action on the glass vessels from the free N.A. in the Nit. Mercury. Had Nit. mercury in excess. Washed the grey precipitate and dried it. It was then a yellow substance entirely volatile by heat from Plat. foil. Heated in a glass tube, water rose, then red vapours of Nitrous acid, etc.; then a red sublimate, at which temperature something remained which by higher heat fuzed, effervesced; when cold it became a white solid.

1411. All together the precipitate seems to be very complicated, containing Sub. nitrate and Fluoride of mercury with fluate of Potassa, etc. etc. It does not decompose by heat into useful results.

1412. The washings of this precipitate were evaporated in a platina crucible, but became too dry; had been probably too hot in my absence. The residue washed with water gave a yellowish insoluble matter and a solution containing little or no mercury; the heat has perhaps been enough to decompose the excess of nitrate.

1413. The dry yellow residue requires a high temperature for its final disappearance. Some red sublimate and some metallic mercury appeared. Nothing very particular here.

1414. A Solution of Nit. Silver and another of fluoride Pm. added together; a little precipitate of chloride of Silver but none of fluoride of silver, for when filtered no addition of nitrate of silver produces a precipitate. Hence fluoride of silver either soluble or not formed in this case. Filtered, added more nitrate of silver to the clean solution and evaporated the whole in a silver basin. It went to dryness and had been rather hot: it was black:

by standing it deliquesced, and no crystals or precipitate except a little from oxide of silver appeared.

1415. Either there is no mutual decomposition or else the fluoride of silver is very soluble. Must make it from Hydro fluoric acid and moist oxide of silver.

1416. As the Fluate of Potassa contained chlorine, now tried bleaching effect on purified Fluoride; for which purpose added a little solution of sulphate of silver to a solution of the fluoride. Put this in silver basin and warmed it. Next day there were some good small crystals and crystalline crusts; these were Sulphate of Potassa. The sulphate of silver was very acid. Filtered to remove all the chloride and then acted on this solution by Voltaic battery, having previously added a little sul. Indigo. There was a very feeble effect of bleaching but nothing like what happened before. Fluorine does seem to bleach, but its powers are very feeble; the oxygen from water will do almost as much. Hence correct that notion in part.

1417. Fluoride of lead promises best; have made some therefore and washed it well.

25 JANY. 1834.

1418. In relation to the interfering effects of vapours on the action of Platina, think we may by it preserve metal surfaces from rusting. Put two plates of cleaned Steel into two jars standing in water, but the metal not touching the water. Put into one jar a vessel with a little oil of turpentine and then left them, hoping to find hereafter that the presence of the vapour of oil of turpentine would preserve the steel from rusting, though in a very damp atmosphere. (1476).

27 JANY. 1834.

1419. *Fusion of Calomel under pressure.* Put Calomel into a white glass tube, pretty thick and hermetically sealed up; gradually heated it in a furnace. It was just on the point of fusing, and did fuse into a liquid portion, as the glass softened and began to swell. It became very dark, almost black, at the part where it fuzed, but as it solidified the yellow and white appearance was resumed. On opening it a little metallic mercury and corr. sub.

was found and it appears resolvable at the fusible point into metal and corr. sub. This metal would interfere with experiment of con-duction and decomposition.

1420. The calomel was dingy in colour when first put in and the quantity of mercury very small, so that it is just possible it may have been in a reduced film on the surface of the crystals in the first instance.

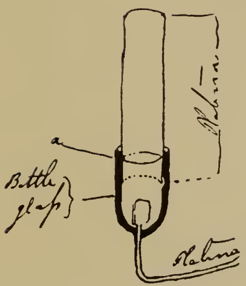
1421. Tried a solution of prot. acetate made with acetic acid and calomel acted upon by Potassa, but there was much hydrogen evolved at the N. Electrode and the solution would not answer any useful purpose as giving the equivalent of mercury for the current passed.

1422. Expt. of conduction and decomposition on *Corr. Sub.* Put Corr. Sub. into a tube with 2 platina wires hermetically sealed, etc., then made it part of a circuit with the Volta Electrometer and melted the Corr. sub. As soon as it was fused, there was a current passed and decomposition in the Volta Electrometer occurred. I think also that there was decomposition in the Corr. Sublimate. But the current was very feeble to what would have passed if an ordinary proto chloride had been present.

29TH JANY. 1834.

1423. Have prepared *fluoride of lead* from solutions of fluoride Pm. and Nitrate of Lead, the latter in excess. Washed it well: it was a white insoluble substance. Fuzed it in a platina tray; it gave off a little pungent fume but no nitrous acid or vapours that I could see. Hence I trust it is a true fluoride of lead.

1424. It fuzes at a fair red heat, becoming clear and colourless as water; not dark coloured as the oxide but more like the chloride in appearance. It fuzes suddenly, not softening first; hence bubbles from it probably break easily and well at P. Electrode. It congeals into a white solid semi opaque substance, very much resembling chloride of lead in its habits.



1425. A tube was now prepared, thus, as is given here partly in section. A platina wire with a platina knob at the end was fuzed into a short piece of bottle glass tube (the sketch is about full size[1]). A platina tube was made out of platina foil, soldered

[1] The printed diagram is $\frac{3}{4}$ the linear dimensions of the original.

with gold, so as to fit inside the glass tube, and put a little way into it, after which by the blow pipe the glass was urged against and fuzed to the platina. Enough fluoride of lead was put in to fill the compound tube up to the mark *a* when fuzed, so that it touched both the lower edge of the platina tube and also the knob of platina at the bottom.

1426. This tube was arranged over a spirit lamp and connected with a Volta electrometer and wires to communicate with a voltaic pile. The Platina tube was made *Positive* and the wire below *negative*. When the fluoride was fuzed, good action took place and I could see down the tube plenty of gas or vapour evolved at the P. Electrode. But the heat was not quite enough nor quite steady enough to keep up a very regular action, and I was afraid to raise it too freely because of the quick action of the hot fluoride upon the glass.

1427. Although the gas or vapour at the P. Electrode came off irregularly and soon mixed with air in and about the tube, I was able to make the following observations. It must I think have been *fluorine*.

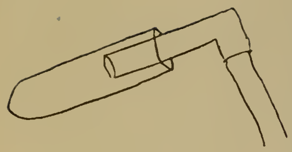
1428. It was colourless and transparent. It did *not* produce fumes in the air like hydro fluoric acid, but when an open glass was held over the end, thus, fumes at the aperture above were produced; but at the same time the spirit lamp flame was near to the lower aperture and a part of its products going in and mingling with the Vapour. I *think* the fumes were hydrofluoric acid, the hydrogen being derived from the spirit lamp flame.

1429. The transparent fluorine vapours were pungent to the mouth and nostrils. They reddened litmus paper but did not bleach it. Paper blue by sul. Indigo did not appear bleached by them whether moist or dry.

1430. On trying a glowing point of carbon in vapours, sometimes it seemed to brighten a little as if oxygen present, or as if fluorine acted as oxygen (but the heat in the tube might do that at the mouth); and at others it seemed to extinguish the spark.

1431. A little copper leaf tarnished in the fumes. Silver leaf also tarnished and looked dull afterwards, but I could dissolve no fluate off from it, nor did S.A. or M.A. give any indication of fluorine or silver. The expt. must however be repeated, for

as a fluoride of silver (soluble) can be formed, if fluorine does not act on silver, then probably the fluoride decomposable by heat and thus obtained the substance.



1432. The vapours being received into glass tubes, either from the platina tube delivering upwards or from another put on to it delivering downwards, acted upon the glass, rendering it white and opaque at the place. Hence *fluorine* acts at once on glass as well as the *Hydro-fluorine*. The part affected was always upwards from the delivering mouth, indicating that the fluorine was a light vapour or gass.

1433. I had to cease these observations now, for the fluoride had by eating the glass began to leak through and also to pass by the joint between the platina tube and glass. The consequence was that the fluoride thus exposed as a drop and a film on the outside was decomposed and reduced by the flame of the spirit lamp, and the negative wire so alloyed by the lead thus produced that at last it dropped off. The voltaic connexion was therefore broken and all allowed to cool.

1434. Being examined when cold, it was found that the glass was much eaten away by the fluoride and in one place eaten through; in others very thin. The silica, alumina, etc. seemed to have dissolved in the fluoride. On breaking the tube to the bottom, it was found the heat had not been enough to fuze all the fluoride down and that a plug had formed in the plat. tube above. It was found that the Neg. electrode was well alloyed with lead, proving that *Electrolyzation* had gone on well and regularly. This alloying did not extend through the wire to the *outside*; the wire there had been alloyed by lead from the fluoride reduced by the flame.

1435. The inside of the Platina tube which had been pos. did not appear affected to any extent, either by the gaseous fluorine or where the tube actually formed the P. Electrode.

1436. Hence it would appear that at this heat both Platina and silver are untouched by fluorine.

1437. Fluorine seems to have less affinity for Platina, Gold, silver, etc. than chlorine, iodine, etc.; and this contrasts curiously and importantly with its affinity for Silicon, Boron, Calcium, etc. etc. etc. I begin to suspect that its affinity for *hydrogen* is stronger than that of *chlorine*, *iodine*, etc., or perhaps even *oxygen*.

1438. Have been distilling Hydro fluoric acid in a Leaden vessel. I diluted the Sul. acid first a little and examined the fluate of lime, which had sulphate of Baryta in it, etc. On mixing the materials there was much action; carbonic acid was evolved and also silicated fluoric acid. After a little delay, soldered up the retort and distilled carefully into water in Platina vessels. Gas continued to come over all the time in moderate quantities; it was Carbonic acid gas. At first silica came over, but the second portion of water did not cause any precipitate and the third and fourth portions, I think, were nearly free from it. A little sul. hydrogen came over towards the last.

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1439. Experimented on the solution of Silicated fluoric acid and the latter purer portions of hydro fluoric acid, to see if the oxygen at the P. Electrode would be deficient and so indicate a solution of fluorine. The acids examined by sul. silver did not appear to contain any muriate. See expts. (   ).

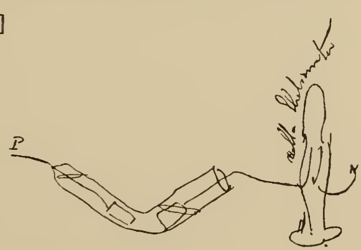
1440*. At first the silicated fluoric acid solution was used. It appeared to have no action on the glass during the experiments. Nor was there apparently any action on the platina at either pole. A little silica separated at first but it was of no importance. Hydrogen was evolved at the N. Electrode and Oxygen at the P. Electrode. It had the electric *smell* I think. A little piece of litmus paper put into the acid was not bleached; being suffered to remain in during three experiments, it communicated a rose colour to the acid, but there was *no appearance* of bleaching. The results of gas in three experiments was as beneath. Therm 54°. Bar. 30·3 inches.

Gas in Volta Electrometer	in tube: equal to
2·14 c: i:	25 = 0·71 of c: i: × 3 = 2·13
2·12 c: i:	25 = 0·71 „ × 3 = 2·13
2·10 c: i:	25·5 = 0·73 „ × 3 = 2·19

Here therefore the oxygen from the Sil. fluoric acid solution was the same as that in Volta Electrometer.

1441. Now experimented with the last portion of fluoric acid

* [1440]



collected and which contains little or no silica. It conducted very much better than the former acid: the gaseous results were

Gas in Volta Electrometer	in tube: equal to
2·1 c: i:	24 = 0·67 of c: i: × 3 = 2·01
2·1 c: i:	22 = 0·61 „ × 3 = 1·83
2·1 c: i:	22 = 0·61 „ × 3 = 1·83

Here the oxygen seems less than should be, but the difference is only small; so that may be due to other causes than a solution of *fluorine* being formed. The acid did not bleach a piece of litmus paper put into it during the expts.; it only reddened it, and even the red colouring matter dissolved in liquid did not *bleach*. Hence also no appearance of solution. Hydro-fluoric acid does not seem to have the same decomposing habits as Mur. acid or hydriodic acid, etc. etc.

1442. The oxygen at P. Electrode had very powerful *smell of Electricity*. Rather doubt smell of that from *Sil. fluoric acid.*

1443. During the experiments and especially in the second and third, there seemed to be much action on P. Electrode and deposition of metal on N. Electrode. On examining this metal it proved to be lead, perhaps alloyed or combined with silicon or fluorine; am not quite sure. It was derived from the glass, for the hydrofluoric acid had acted regularly on the glass, dissolving every thing and therefore giving no appearance by opacity of this action, and the silica, the alkali and the oxide of lead had all been brought into solution. It was this solution of lead which in the second and third Expt. gave such abundant precipitates at the N. Electrode, and I found that the precipitate at the P. Electrode, or the blackening there, was not due to the action of fluorine, but the formation of per-oxide of lead, as in former cases, by a secondary action.

1444. This formation of Per oxide will account for the deficiency of oxygen in the second and third expts., by which time more lead had been brought into solution.

1445. Fluoride of lead evidently soluble in excess of hydro fluoric acid.

1446. Hydro fluoric acid had not decomposed here as muriatic

acid would do. Apparently very strong attraction for the hydrogen by the fluorine.

31 JANY. 1834.

1447. Prepared Oxide of Silver from sol. Nit. of Silver and caustic potassa in excess; it required boiling to decompose thoroughly all the nitrate. Washed the oxide well in water—it required heating and much stirring to remove all the alkali.

1448. As the last washings, though free from alkali, seemed to contain a little silver, examined them. When filtered and perfectly clear, if tested by Mur. Soda, there was no trace of silver, not even when a drop or two of nitric acid added first to decompose any supposed alkaline compound. Still this filtered liquor tasted metallic, and when evaporated, little films of reduced silver arose, the solution became turbid and left a small quantity of residue, black, and with little water giving a very alkaline solution. A little N.A. added dissolved the black residue and all, *but still* Mur. Soda *did not* shew silver. What can this black matter be? Is there a compound of alkali and an oxide of silver, soluble, etc. etc.? Must examine this hereafter.

1449. The films of silver spoken of above gave different colours by reflexion of light. Does not this imply transparency? For is it not a case of thin plates, and the action there is of necessity dependant upon transparency. A good case of transparency.

1450. The oxide of Silver prepared as above affects turmeric paper powerfully, as if alkaline. Thought this might be from decomposition of a chloride of potassium or sodium in the paper, but is not, for an infusion of the paper gave no precipitate with Nit. Silver. Is an effect of the oxide.

1451. Formed a *fluoride of Silver* by dissolving this oxide in the purest portion of the hydro fluoric acid of the 29th instant in silver vessels.

1452. By having excess of the oxide it was easy to render the acid perfectly neutral, i.e. the clear solution did not render litmus paper red, but green at first, and afterwards by reduction of silver it was more discoloured. The solution also reddened turmeric paper strongly, but in an hour or two this also changed still further. This liquid held a fluoride in solution.

1453. On pouring off the liquid, and after it had become quite clear by deposition, evaporating it in a silver dish, it became concentrated, evolving no acid fumes; and at last deposited orange films and soon after began to crystallize at the bottom regularly but small, and at the surface in films. Stirred the whole until very nearly dry. It became black and I suspect two fluorides are present, but perhaps it is only the hydrated and anhydrated states. I unfortunately got some sand into this batch just as it was finished off to dryness.

1454. A portion of the fluoride without sand, heated on platina, after becoming anhydrous, fuzed, becoming a deep coloured liquid with a metallic appearance. On cooling it solidified, crystallizing —it could be fuzed and solidified many times without further change. Upon being heated (it was in air and by spirit lamp flame), it was decomposed, boiling at the time, and left metallic silver. I suspect and hope that in this expt. pure fluorine was set free, and then using platina or silver vessels, I shall catch it.

1455. I begin to think Mercury will hold fluorine.

1456. In making the fluoride the whole of the oxide does not appear to dissolve, or rather a black matter is formed remaining undissolved even when excess of hydro fluoric acid present. Still, I think that a part of it dissolves and that it is probably one of two compounds. I poured the acid solution off a portion of this, washed the latter well in water, and intend examining it to-morrow if the Excise will let me. I will call it (A).

1457. The acid solution poured off from this was not [? now] saturated with more oxide and evaporated at once. It gave off much hydro-fluoric acid fumes and left .

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1458. The black insoluble powder (A): much of it is mere organic fibre, as dust, etc., derived from different accidental sources. Much of it is chloride of Silver. The rest may be oxide, but as it did not dissolve in ammonia, nor fuze by heat as chloride or fluoride would do, it may be some metallic impurity. Its quantity when dry was small, as indeed was the whole quantity of (A) at first, though it looked large when moist.

1459. Examined the soluble ordinary fluoride of silver: neutral,

etc. to litmus and turmeric paper. When evaporated to dryness on platina, it evolves acid fumes and gives a dry body—deliquescent. But at same time is formed a golden yellow crystallized body, not deliquescent and not soluble in the general solution. These by addition of water became dull and lead coloured, but by re-evaporation gold coloured again. When water solution was *removed*, they became permanently dull and did not recover gold colour, etc. by drying and evaporation. But adding a little of the soluble fluoride they then by evaporation became yellow and gold coloured, etc. again.

1460. I found the silver dish in which the soluble fluoride had been evaporated, and in which much gold coloured crystals had formed on the silver, very much eaten. Another silver dish had been by similar treatment almost eaten through to form the gold coloured substance. The solid parts of the mass of insoluble matter, when scratched by a knife, felt malleable, etc. like chloride of silver, but gave *gold coloured metallic streak*.

1461. Think that a sub fluoride of silver is formed and that by water this is resolved into the soluble fluoride and metallic silver. The sub fluoride being the insoluble crystallized yellow metallic looking body. When well and repeatedly washed the insoluble body left is *metallic silver*.

1462. These relations of silver and fluorine very curious and agree well with notions of fluorine being analogous to chlorine, etc. etc.

1463. Probably advantageous to add pulverized silver when evaporating soluble fluoride of silver in silver basins.

1464. Hydro fluoric acid can dissolve oxide of silver up to a certain point, but after that only metallic silver can be taken up. This point is where the oxygen of the oxide is equal or equivalent to the hydrogen of the hydro fluoric acid.

4 FEBY. 1834.

1465. May evap. soluble fluate of silver to dryness and fuze and even decompose the resulting sub fluoride on platina, for Nitric acid will dissolve off the metallic silver left and the platina seems to be uninjured, provided the heat of fusion has not melted the silver in to it.

1466. The fluoride of silver decomposes well in the air by heat, and I thought it would give me fluorine and silver, but begin to suspect that air or rather the moisture in the air is essential to the decomposition. Proceeded to test this point.

1467. Wrapped up in one plat. foil and heated in the air. Here it decomposed, but still air might have entered; besides, the fluoride very fusible and flow over the surfaces and so come to the air.

1468. Wrapped up in platina and heated in a glass tube. Here decomposed, but the tube too open to be satisfactory.

1469. Wrapped the hot dry sub fluoride up in two pieces of platina very close, put the parcel into a green glass tube and heated it red hot in fire, so as to avoid to a certain degree a hydrogen flame or source of heat. It was well red hot so that glass softened. When cold enamined. The sub fluoride was found to have run from its original place among the folds of the platina, but there was plenty of it *undecomposed*, so that red heat had not been sufficient *per se* to decompose it.

1470. The pieces of this packet, which had been fingered by pulling to pieces and examining, were now put into a glass tube and heated by a spirit lamp. Immediately it decomposed, shewing that much fluoride had passed through the fire heat unchanged, and that the moisture given by handling and in the air of the tube had now favoured decomposition. The glass was much acted upon and silver was reduced.

1471. In this last expt. what seemed to be moisture rose and condensed upon the cooler part of the glass in little dew drops. It did not seem to act upon the glass. It could be moved about by a platina wire like moisture. When a particle of water was put (by the wire) in contact with a small drop of this liquid, there seemed to be strong action and the glass all round the place was instantly acted upon and rendered white and opaque, not from deposition only, for the wire could not remove it, but apparently from actual corrosion by a vapour suddenly evolved from the mixed drops.

1472. Hence this liquid very peculiar. It was hardly likely to be hydro-fluoric acid of the ordinary kind—its easy condensation and non action on the glass is against that notion. I must look after it.

1473. I believe that the reduction of the fluoride in the glass tube depended upon the presence of moisture, but I am not quite sure of this. It may have been due to the presence of oxygen—but then a new oxy-fluoric compound produced. Think it must have been the water.

6 FEBY. 1834.

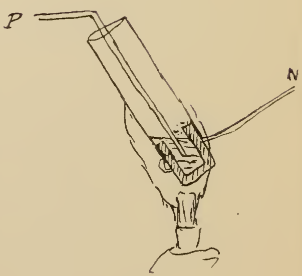
1474. On the red precipitate at the P. Electrode in solution of Zinc. See 7th Jany. 1834 (1338). This precipitate has been washed and dried. It is a dark red brown powder. It does not fuze by heat on plat. foil and is not brown oxide of lead. In Mur. Acid it dissolves, forming at first a brown solution which by standing or heat became nearly colourless, having only a pale yellow colour. On evap. to dryness, it left yellow substance—deliquescent—not having a strong taste. This in water gave clear solution, which by different tests indicated first a little iron—then a little lead—then manganese; and with caustic potassa it formed a fine chameleon.

1475. The substance consisted at first of per oxides of lead, iron and Manganese, the latter in greatest quantity. These have all been formed by secondary action on the impurities of the zinc solution. There is no new substance.

7TH FEBY. 1834.

1476. Have had two pieces of iron in two jars of common air standing over water for several days past. In one of the jars was a cup with a little oil of turpentine. Both pieces of iron have begun to rust and I think about equally. The temperature seems to have no effect (1418).

10 FEBY. 1834.



1477. Worked for fluorine. Used a strong small platina vessel like a little crucible, and having made a tube of platina soldered by gold, soldered it on to the former. Adjusted a thick Plat. wire P so as to go into it, touching nowhere but dipping into fuzed fluoride of lead, which was kept in that state within by the heat of a spirit lamp without. A volta-electrometer was attached

at N and the neg. pole of battery connected there, whilst P was rendered positive.

1478. There was plenty of action. Lead was precipitated on the platina, alloying with it, and gas evolved at the end of the wire P; for this gas or vapour the upper part of the platina tube answered as a receiver. It must have been fluorine.

1479. The brightness of the inside of the platina tube caused plenty of light to shine down it, and it was very easy to see the state of things within.

1480. The fluorine was colourless and transparent, a gas or vapour boiling up abundantly from the P. Electrode and not fuming in the air—it was very pungent. It seemed to extinguish a coal, but burn with hydrogen. Sulphur did not burn in it, but boiled and blackened. It acted on a clean bright silver wire, rendering it cloudy. It acted rapidly on glass. It acted on copper leaf, but I could not get the metals to burn in it, even zinc. The quantity was too small.

1481. Potassium would probably?

1482. I could not try its bleaching powers, for the paper charred, or if wet, reddened.

1483. Must try Pm. and Phos. in it.

1484. After a while the side of the plat. tube ran, from its having been eaten through and fuzed by lead reduced upon it as the N. Electrode. The expt. was therefore stopped, but not before as much as $8\frac{1}{2}$ cubic inches of O. and H. had been evolved in the Electrometer. This is equal to rather more than $\frac{2}{3}$ gr. water decomposed. Hence as much as 8 gr. nearly of lead must have been reduced and the proportionate quantity of fluorine set free.

1485. On cutting open the thin part of the tube, the action upon it of lead reduced was very visible, but there seemed to have been very little action, if any, on the sides by the *fluorine*. On examining the end of the thick platina wire which served as the P. Electrode, it was found rounded and dissolved in part, so that the fluorine evolved against it had dissolved some; but the action was as nothing by comparison with what a *chloride of lead* would have caused. Hence relative position of chlorine and fluorine; and indeed chlorine expells fluorine, and hence perhaps a chemical process even upon fluoride of lime.

1486. On melting out the rest of the fluoride of lead, it was found discoloured by mixture of a little fluoride of Platina, but otherwise unchanged. At the bottom of the thick vessel of Platina was seen the portion of fusible alloy of lead and platina. This is very definite and may almost be melted from off the platina, as I have seen in other cases.

1487. Must now lay this subject aside for a while and go to the trough.

1488. Oppose equal decomposing plates to equal generating plates.
1489. Alter size of the Poles.
1490. „ strength of the solution.
1491. „ nature of the decomposing body.
1492. Compare intensity of one pair or of two to machine electricity intensity.
1493. *All* decompositions require probably the same intensity if relieved from secondary action. No.
1494. Arrange two simultaneous decompositions of different kinds.
1495. By use of pure or amalgamated zinc, may have a battery not active when not connected—active when connected—here all the Electricity used and accounted for that is produced by chemical action. The reduplication being to produce intensity.
1496. This definite production of electricity a proof that the electricity is *due* to chemical action in voltaic pile, for 34 grs. of zinc and 9 of water, etc. produce a definite quantity, etc. etc. etc., as the argument runs.
1497. This may cause the Voltaic trough to be an economical mode of preparing some things, as Barium, etc. etc., or even Pm., for get it by the equivalent of Zinc used.
1498. But will shew that plan to decompose proposed in some other cases bad, because only the equivalent could be obtained, however many series of plates were used.
1499. In De la Rive's expts. with plates, etc., besides the constant effect of each additional diaphragm, there was also a counter effect of the elements arranged at the plates by the current, and this cause another ratio of the sum of effects.
1500. That this effect of arrangement is great is proved by the improvement in a trough produced by stirring the acid between the plates; the increase of power is great.
1501. When the charge in a battery is an acid able to act on the metals though no current occur, as N. A. for instance, the quantity or intensity (consider the two here) above the unit of effect is

discharged laterally. Refer to tuning fork vibrations, etc. etc. for a figure.

1502. The power of decomposing water a good *unit* of intensity in voltaic apparatus. Query its relations to common intensity. Make this out by reference to diverging power of a battery with a certain charge—a *water* or *acid battery*.

1503. Daniell called on me to-day to ask me about my views of the elementary expts. of single pr. metals and the relation to pile, etc. etc., and if it had not occurred to me whether he might work it. I told him my views, and wished him to work cotemporaneously with me. He behaved very generously, leaving it open to me alone. But if another catches my idea, and works it out before I can write my paper, I shall always regret that Daniell has given way to me, and that another should come before him. Must leave fluorine and hasten this matter of the VOLTAIC PILE. I shewed Daniell my preparatory notes for the paper.

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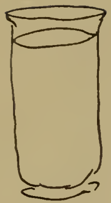
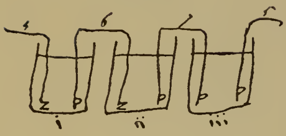
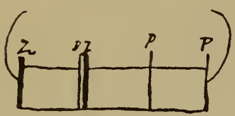
1504. Expts. on the generating plates and the intensity of the current they produce. The existence of a current was ascertained both by the chemical action at the Platina plates and also by a galvanometer having two needles. The position of the latter and of its wire was such that, when influenced by the earth only, the upper needle stood as in the position *a*; but that when the ends were zinc Z and Platina P, and dipped in dilute Sul. acid, the deflection was more or less in the direction *b* indicated by the dotted line. In the expts. to be described Z was always made zinc, or at least the more oxidizable metal, and P always Platina.

1505. Numerous arrangements of plates were now made which will be understood from the following description. *a* and *b* are plates of platina about 2 inches and ½ long and ½ or ⅔ of an inch wide. These are soldered on by gold to a Platina wire *c* about 8 inches long, so that *c* can be bent and the plates *a*, *b* inserted, either into the same glass or into contiguous glasses containing acid or other solutions, so as to build up a form like the *couronne des tasses*.

1506. Other pairs of plates had one platina, another zinc—or were platina and amalgamated zinc—or platina and copper, the metals







associated being different but the form the same. The cups were of the form in the margin and of glass, that the insulation might be perfect. I intend representing the different arrangements thus[1]; for instance, where i, ii and iii are three cups containing acid or other liquid to be described, 4 and 5 the zinc and platina plates just referred to as connected with the galvanometer, and 6 and 7 pairs of plates like those just described, 6 being platina at one end, zinc at the other, and 7 platina at both ends. The terminal plates, as 4 and 5 here, shall always be in the same relative position and order to the galvanometer as that described in the first expt. of this day.

1507. It will easily be seen that the arrangement of cups above is in effect exactly the same with the following, where a glass trough consists of three cells containing acid, at Z a zinc plate terminates the trough, at P one of platina, at PZ a compd. plat. and Zinc plate divides the acid, at P a platina plate only; and so on for all other arrangements of cups and plates.

1508. Some Sulphuric acid was diluted when cold; it had a Sp. Gr. of   and acted powerfully on ordinary zinc, evolving much hydrogen, but not on amalgamated Zinc, except very slowly. This was put into glasses and arranged with a pair of platina plates and the final plates, thus*. Glass rods were put into the cups between the plates to keep them from touching each other.

1509. In this case there was no sensible current. The middle plate of Platina had prevented all transfer or current of Electricity, even to the galvanometer.

1510. Removing A, I used in its place a mere platina wire, but the effect was exactly the same. Hence making the interposed plate touch by small surface, whilst evolving plates are large, causes no difference: no current.

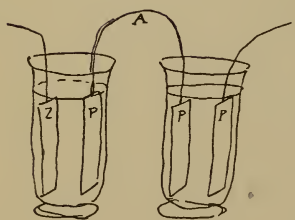
1511. I then used very large plates at ends of A and mere wires of zinc and platina at the extremities, but neither did this alter the effect.

1512. Variations of this sort therefore, as to mere quantity of evolving surface or mere increase of facility at transmitting surfaces of intervening plate, are of no consequence.

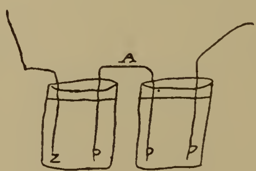
1513. It is needful that in these and similar experiments care

[1] Refers to diagram in margin.

* [1508]



or thus



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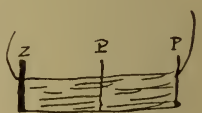


should be taken that all the Platina plates are equally clean and pure, or else they by their differences cause currents of electricity which can pass.

1514. Now used two electro-motive pairs of plates instead of one and one interposed decomposing platina pair, thus*.

There was at first a little deflection, but it soon gave way and the needle stood uninfluenced by any current; nor was there any gas or other signs of current at platina plates. So that it would seem that one decomposing plate of Platina having *no affinity* for the bodies evolved was able to stop the current which two decomposing pairs of zinc and platina plates tended to produce.

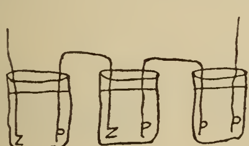
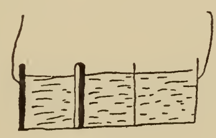
1515. When a third pair of electro motive plates were used, thus†, then a permanent current appeared both by the galvanometer and by the bubbles on the platina plates. Its direction being as if all the plates except the extreme ones were removed.

1516. Three series of decomposing plates therefore are able to produce a current having intensity enough to overcome the affinities of oxygen and hydrogen in cell 1. Two have not, but seem almost equal, if not quite; perhaps just balanced.

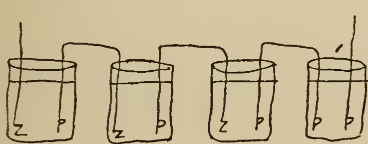
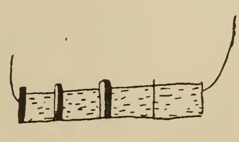
1517. Then three pr. of electro motive plates were put against two decomposing plates of platina, thus‡; but there was now no deflection—no decomposition—no current. The two mere decomposition required in cells 1 and 2 were more than could be effected by the current having an intensity due only to three zinc decomposition in 3, 4 and 5.

1518. Then *four* pair of Electromotive plates were opposed to two decomposing platina plates, thus§. Here there was no current —no deflection—no decomposition.

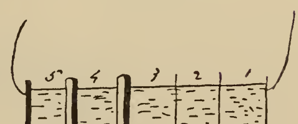
* [1514]

 or 

† [1515]

 or 

‡ [1517]

 or 

§ [1518]

 or 

1519. Then tried *five* pair of Electromotive plates against two decomposing plates of platina, as thus*. This gave a feeble current; there was both deflection and decomposition. The current was however much less than if all the intermediate plates were taken away, for on putting then into acid, 6 inches apart, very strong resulted and good decomposition as shewn by the gas on the platina.

1520. Hence five pairs contrasted with two decomposing pairs do not give a power equal to one unobstructed pair.

1521. In all these experiments the Zinc plates were amalgamated so as not to act except when the current was passing. But must try whether the ratio of effect the same with unamalgamated plates.

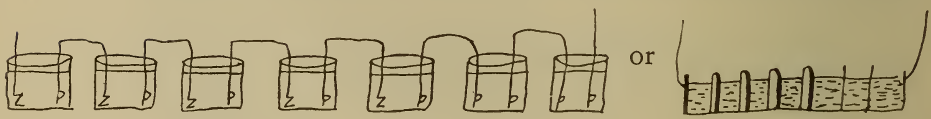
1522. I now changed the nature of the intermediate decomposing plate, using an amalgamated zinc instead of a platina plate, thus†.

1523. There was a strong current just as if the middle zinc plate were away, for it held the needles at right angles to the coil and decomposition was plentiful on the platina plate. Hydrogen was evolved at 1 and also at 2, but not at 3 or 4, the oxygen being rendered there and uniting to the zinc. There were a few bubbles on 3, but these were due to the direct action of the zinc on the acid, as I found by a piece left in the acid, and yet the circuit broken.

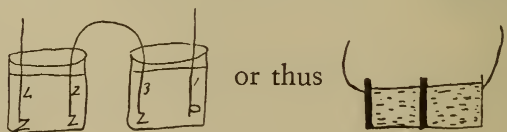
1524. Now two prs. of zinc intermediate plates were used in place of one, thus‡. This gave a strong current having the same direction as in former cases. It still was much weaker than when only one intermediate.

1525§. Now used three intermediate zinc plates to single voltaic pair. Still a current and pretty strong, but no[t] so strong as if from P to Z1 had been merely the dilute Sul. Acid uninterrupted.

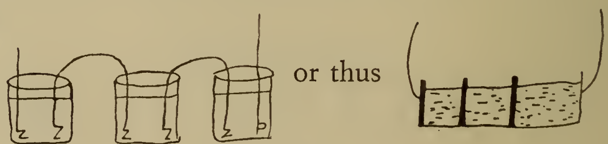
* [1519]



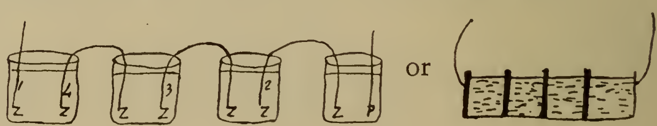
† [1522]



‡ [1524]



§ [1525]



The intermediate series therefore do retard each the current and its effect. There was evolution of hydrogen on the P and also on the zinc ends 2, 3 and 4, in apparently equal proportions.

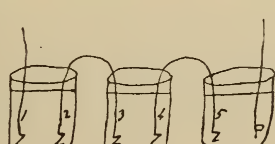
1526. These zincs were all amalgamated.

1527. That each zinc interposed should retard the current seems to be because each zinc wanted a little more power under the circumstances to enable it to decompose Water, and this power the current gave it, loosing of its own force. But still the point recurs; how is it that the zinc *in contact* with the platina has such power as to decompose water and half succeed in decomposing water at a second surface?

1528. How very needful the current is to the decomposition in a single pair of plates. How very needful for the existence of the current *is decomposition* in the cases where the intervening platinas are used. But as they cannot be cause and effect to each other, what is the common origin or cause of both? Must make this out. It is of no use continuing to suppose one as producing the other in either order.

1529. Now repeated the last expt. but one, i.e. the one with two interposed pair of zinc plates, but used them *un-amalgamated*, thus*. The end 1 was amalgamated, but 2, 3, 4 and 5 were not, or in the second figure *a* was amalgamated but *b* and *c* not. In this case the current was far more intense than when the intermediate zinc plates were amalgamated, and was as intense, I think, as if they had been away. Perhaps even a little more so. The fact shews that the nature of the interposed metal very important. For Platina interfere very much with the current. Amalgamated Zinc much less and Clean Zinc still less. This is consistent with the relations of the three to oxygen; and with the platina it is necessary to expell it per force; with the amalgamated zinc the affinity is not quite enough but wants help from the current; but with the clean zinc it can at once decompose the water; requires no help from the current. There is therefore no reaction on it; no retardation, but rather an increase of power, because zinc 5 unamalgamated is in more powerful electro motive relation to the platina than the amalgamated zinc 1.

1530. As the unamalgamated zinc acts by itself, decomposing water, it is probable that its interposed plates produce no retarding

* [1529]  or 

effects, except perhaps as to length of humid conductor involved; for the decomposition is continued on from one to the other without the aid of the voltaic current, receiving only direction of the elements from it.

1531. This unamalgamated zinc was acted on very strongly by the acid, evolving much hydrogen in a short time.

1532. These cases of retardation seem to indicate that the retarding effect is exactly equal to the affinities to be overcome, and that where the current is deficient in power it may be helped by calling in chemical aid; the former being replaced or made up by an equivalent of the latter. Does not this shew a remarkable *identity*? Further, the original current is in its power in proportion to the chemical affinities producing it, again shewing identity.

1533. The differences between the different interposed plates seem also to refer the electricity decidedly to chemical action.

1534. The whole arrangement seems beautifully to shew the antagonism of the *chemical powers* at the Electromotive parts with the *chemical powers* at the interposed parts. The first are producing electric effects; the second opposing electric effects, and the two seem equipoised, as in a balance, and in both cause and effect appear to be identical with each other. Hence chemical action merely electrical action and Electric action merely chemical.

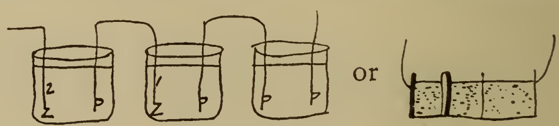
1535. That the chemical and Electrical action are convertible shewn by the condition of action and non-action at the interposed plates, according to their nature and number. It seems that to balance the power of a battery current, all that is required is to interpose plates until the sum of the chemical action against them is equal to the sum of those in the battery. If, as with platina, all the power must come from the current, then few will do. If, as with amalgamated zinc, but little is required from the current by each, then many are required, and so on.

1536. I am continually wanting a clear definite view of the actions in a single voltaic circuit.

1537*. To ascertain the superior electro motive power of unamalgamated over amalgamated zinc, now experimented to see if two pair of it, and Platina, were also neutralized by one interposed platina plate.

1538. There was a very trifling current at first, but that soon

* [1537]

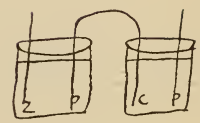


ceased and the tendency seemed balanced. Perhaps there was no reason to expect that the abstraction by sufficient affinity of oxygen in two generating cells was more than sufficient to counterbalance the two separations of oxygen and hydrogen in one cell; since the attraction of oxygen for hydrogen must be the same as of hydrogen for oxygen.

1539. There was much gas evolved at 1 and 2 zincs, but not at any of the platinas. There was no current.

1540*. Then tried the electro motive power of pairs of Platina and copper in this pure strong Sul. Acid 1336. But there was no sensible action either at Galvanometer or by appearance of bubbles on the platinas. Probably a trifling action, but nothing of importance to me; neither was there chemical action.

1541. Concluded a pair of platina and copper plates would therefore neutralize the current of a pair of platina and zinc plates, thus; and found this was the case, notwithstanding that the Plat. and copper were in the order to form a current like that of the platina and Zinc. This may also well illustrate the way in which bad plates, or a weakly charged or exhausted trough, or one with copper on the zinc, interferes and neutralizes the action of the other good.



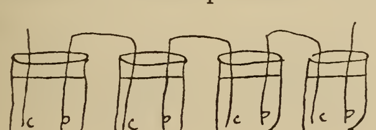
or

1542. Never put weak and strong troughs in succession again. Better to leave out the weak. Have more power.

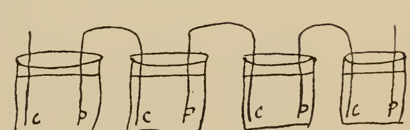
1543†. The pile was now reconstructed with the four pair of copper and Platina plates, but a little nitric acid had been added to the Sulphuric acid with which the glasses were charged. The arrangement was now very active and gave a powerful current to the galvanometer; one pair being as powerful in that respect as the four, according to the well known law. But the important point is to observe that, the moment the chemical action upon the metal could go on, that moment a current began. The oxidation of the metal is not now from the water but from the nitric acid.

1544. Is it not possible that the electricity is evolved between *the metal* and what it combines *with*, i.e. in this case the oxygen; and that the previous condition of that oxygen is only of consequence as offering a certain quantity of chemical effect to be overcome, and a relation to other particles which can favour or allow of

* [1540]

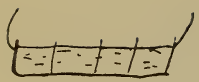


† [1543]



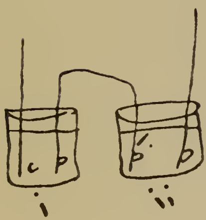
transmission by Electrolytic action across the humid conductor? The combination of the acid with the oxide produced seems to have no *direct* influence in producing electricity, for the Electricity produced is *equivalent to the* OXYGEN combined with the metal.

1545. In that case, look to the particles of the metals for the Electricity evolved, and in that view remember their conducting yet indecomposable state, which no other set of bodies have, i.e. no other set than those which, like the metals, are needful in the voltaic pile.



1546. Took out the three intermediate pairs of copper and Platina, and using the same Sul. Nitric acid, interposed a single pair of platina plates between the extreme copper and platina plates, thus*; and found that a strong current passed. Then used two interposed platinas, thus†. Still a current passed. And even when three interposed platinas were used, still a current passed, though weaker much than in the former cases.

1547. Must remember that Nitric acid present in all the glasses, that therefore the copper has not to decompose water, but finds oxygen ready to combine freely with it; its powerful affinity is not therefore opposed by another powerful affinity and the electricity evolved is of proportionate intensity. On the other hands, the interposed plates afford *less* obstruction than if in Pure S. A., for there is either no need to decompose water there, or if there is, the hydrogen has an affinity presented to it at the N. Electrode. Probably get some means here of deciding whether N. A. is decomposed primary or secondarily, and if the latter, how it favours.

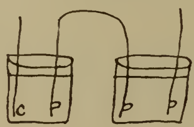


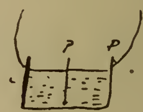
1548. Then again in this arrangement. When the acid in both i and ii was Sulphuric with Nitric acid, the single pr. of copper and platina was able to send a current through the whole. But when the acid in ii was the dilute Sul. acid without nitric acid, so that *water* had to be decomposed there to supply the oxygen at the Platina 1, then *no current passed.*

1549. Hence the chemical relation at the *incidental* or decomposing plates important as to the final decomposition and transfer.

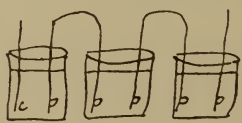
1550. It is curious to observe how the possibility of decomposition against the platina plates in ii *here governs* the evolution of Electricity. I see no positive proof as yet that contact is necessary for

* [1546]





† [1546]

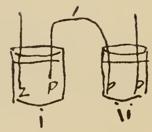


or

generation, only for the conduction and restoration of state. But the current seems necessary for decomposition.

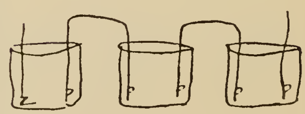
1551. Must consider the case of single decomposition very well and closely,  for that includes the whole. Why is it necessary there should be a discharge of electricity before action can go on? Why not zinc alone decompose, and how is it that in existing circumstances the platina helps?

1552. Made an expt. of transmission through many intervening plates by only one pair of Zinc and platina plates, thus*. But all the vessels contained sulphuric with Nitric acid diluted as before. The current passed pretty strongly through one plate—it passed also through two, not so strongly—and it even passed through three intervening platina plates, but less strongly. Hence to obtain an intense current, a charge with N. A. probably good; but effect here on intermediate was also on dilute N. A.



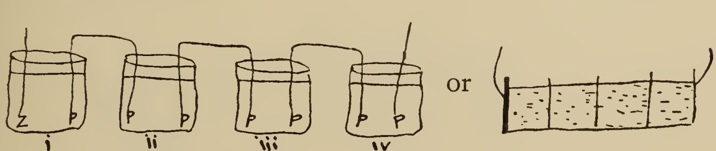
1553. When i contained the S. N. Acid with zinc, the current evolved would pass through the interposed Platina plate 1, though the acid in ii was pure sul: acid diluted, but by no means equal to what would have passed had ii contained S. N. Acid.

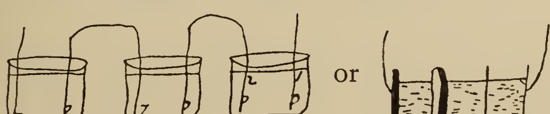
1554. The current could then even pass through two intervening plates, though ii and iii contained sulphuric acid without any nitric acid; but the force was very small compared to that of the current passing when ii and iii contained Sul. N. Acid.



1555. Now used Muriatic acid so strong as to act well and quickly on unamalgamated Zinc, and evolve much hydrogen. Only one generating pair of zinc and platina plates were used. The current could pass feebly by one interposed platina, but scarcely sensibly with two. Then arranged two pair of zincs and platina with only one interposed plate, thus†. There was a feeble current on the galvanometer, but so feeble as not to give any quick signs of bubbles on the Neg. Plat. wires. When P1 made to touch P2 so as virtually to remove the latter, then a powerful current passed and much hydrogen gas was evolved at all the Plat. surface.

1556. Notwithstanding these and all other variations in the intensity of the power, the law of definite action of quantity appears to hold constantly true.

* [1552] 

† [1555] 

$1556\frac{1}{2}$. Mur. acid can give no chlorine to zinc without evolving Hydrogen somewhere. In this respect it is like water and unlike Nitric acid. Hence more like water in its *actions* also and regulations.

1557. The Muriatic acid did not make a much better voltaic battery with Copper and platina than the Sul. acid did.

1558. With respect to the interposed plates, it is easy to pass from them to voltaic pairs of plates, considering first platina plates with sul. Acid and then N. S. Acid, then zinc plates with same variation and then conjoint plates acting of course as battery plates.

1559. Compare intensity of one or of two pairs of plates to machine intensity.

1560. Decompositions cannot all require same intensity of current. Evidently differ in the attraction of the particles united, and also in the attractions of the plates against which their elements are to be rendered.

1561. Try a decomposition of Nitre or fuzed chloride of silver by zingle pairs of plates.

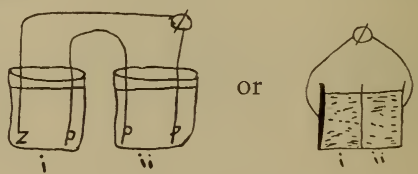
19TH FEB. 1834.

1562. Examined the action of pure zinc on the S. A. diluted; it acted and hydrogen was evolved, but it acted very differently indeed to a similar piece of ordinary zinc; yet on putting platina wire in contact with it, the wire gave off abundance of gas, more than if in contact with the ordinary zinc. Left the acid with both pieces of zinc in. I have no doubt the small piece will be gone in an hour or less. Next morning examined the glass. Of the ordinary zinc only a little flocculi, black (lead, copper, etc.) was left; of the pure zinc above one half was left. That was still evolving gas, and being touched by the platina wire, evolved on it abundance of gas. This was not quite pure, for there hung about it metallic flocculi which proved to be lead. Perhaps, however, they had been dissolved from the other piece and reprecipitated on this.

1563. See De la Rive on pure zinc.

1564*. Now experimented on certain decomposition effected by a single pr. of plates to ascertain whether the intensity produced by one constant pair was enough for *some decompositions and* NOT *enough for others*.

* [1564]

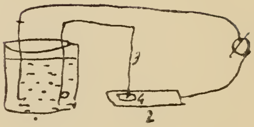


1565. When the same dilute Sul. acid was in both i and ii then no permanent current passed; a little effect when the plates were put in, but no other. But when a little N.A. was put into i, then a permanent current passed, but not very strong. Hence increasing the action in i by N.A. (query whether more s.a. would do), made the current far more intense.

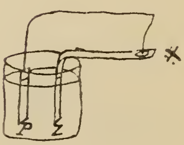
1566. When N.A. was added to ii also, facilitating the decomposition, etc. required to favour current there, then a strong electric current passed. So here a PROOF that a single pair of plates can *decompose* well in a second vessel.

1567. When N.A. was in the S.A. of ii whilst only S.A. diluted in i, then the current.

1568. Now experimented on solutions of hydriodate of potassa, sulphate of soda, etc. etc. etc., thus. i contained dilute S.A. (no N.A.). The zinc plate Z was connected with the wire of the galvanometer ϕ and that with a platina plate 2, on which rested a piece of paper of two thicknesses moistened in the solution to be tried; and upon this the end of P rested, the whole of that, both plate and wire 3, being platina. When the paper at 4 was moistened in solution of hydriodate of potassa, plenty of iodine instantly appeared (as it ought to do) at the end of the wire 3; and when the paper was turmeric paper, corresponding dots marked by alkali were found on the opposite side against the platina plate 2. Hence a single circle will decompose hydriodate of potassa freely, though only S.A. be in i. The Galvanometer swung but very little, the point of contact being very small for the discharge; but the iodide a better test than the galvanometer.



1569. Now arranged so as not to have a single pair in metallic contact. In fact no metallic contact, thus. P was a platina plate made fast to a platina wire which was bent so as to come round and rest on the piece of moistened paper at *. Z was a zinc plate amalgamated and bent round so as to sustain the piece of paper at *. The vessel contained dilute Sulphuric acid without any N.A. The observation was to be made at * and there was no metallic contact here except perhaps the difference of amalgamation at the two ends of the zinc plate.



1570. Neutral hydriodate of potassa in solution both on white

and on turmeric paper at * gave iodine at the end of the P wire as it ought to do, and alkali against the zinc plate.

1571. But when the two plates were made to touch at P and Z or elsewhere, then the counter current was produced due to the direct action of the sol. hydriodate on the two surfaces at *, and alkali appeared against the end of the platina wire. A beautiful reversion this and a good expt.

1572. Sol. Hydriodate of Potassa a substance which can shew decomposition when only one pair of metals and one fluid used.

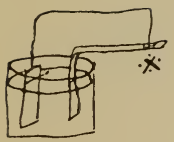
1573. When the paper at * was litmus and dipped in Sul. Soda, so as to repeat the expt. in form as at , no trace of acid appeared at the end of the platina wire in a minute or so.

1574. When the litmus paper was dipped in solution of common salt, still no trace of acid or of bleaching at *. The motive power in the first vessel not sufficient to overcome these substances, which themselves tend to produce a counter current.

1575. Even when the vessel contained S.N. acid, I could not decompose a solution of salt or sulphate of soda in a few moments, so as to obtain results at * at the end of the platina wire.

1576. Using Muriatic acid in the paper I could not bleach at *.

1577. Thinking the amalgamation of the zinc might be irregular and therefore supply a difference at the two ends equal to a contact between two different metals, I dismissed the amalgamated and now used unamalgamated zinc. But I found that now I could not even decompose solution of Hydriodate of potassa at * unless there was a little N.A. in the S.A. of the vessel containing the ends of zinc, and then it would give way. It was then however *a good* case of decomposition, not merely by a *single pair of plates* but even without *contact* of metals. See 1189.



1578. There was a curious appearance and result here. Thus when the cup contained only dil. S.A. there was no action on the hydriodate of potassa at *. But if that cup were removed and another put there with S.N. Acid, then the hydriodate was decomposed. On taking away the second cup and restoring the first the decomposition of the hydriodate *could be produced* and continued for any time. On taking out the plates from the acid and returning them immediately or after 2 or 3 seconds, the decomposition still went on, but on keeping them out 8 or 10

or more seconds, the decomposition ceased and could not be renewed without taking away that acid and using the N. S. Acid for a moment as before, and then things went on. This effect did not seem to be due to the little N. S. Acid carried into the S. A. cup by the wet plates, for a *little* of the N. S. Acid added to the S. A. cup did not give the power of decomposition.

1579. It looked rather as if the particles had been favourably arranged by the passage of the current and kept that arrangement for a while, but whether this species of polarity might be supposed to be in the metal or in the acid or in the Hydriodate under decomposition, I cannot say.

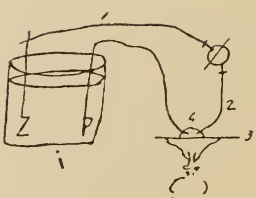
1580. Consider how such polarity would agree with that spoken of by De la Rive, etc. etc. But there the final tendency is opposite to that current which induced it. *See page 345, 25 Feb.*[1]

1581. As to contact of metals not being necessary. Now that I have made and understand the expts. just described, can easily see that the Expts. by Davy of Voltaic piles with one metal and two fluids prove the same thing.

1582. Again, the preservation of copper without contact by Davy another case—if it be a fact. Phil. Trans. 1825, p. 337.

1583. Still, effect of contact seems to be very important in the single circle, but it is the contact between the acid and the metal or rather probably between the metal and the water whose oxygen it wishes to take, but has not affinity enough without the aid of the current.

1584. Now experimented on some cases of decomposition of other substances by a single pair of plates, but where metallic contact was allowed, both the decomposing ends being of platina. Thus. Z was zinc, P to the globule platina, 1 and 2 also platina: the wire of the galvanometer copper: 3 is a piece of florence flask, flat or curved, and 4 any matter fuzed upon it by the spirit lamp and subjected to decomposition by the current from the single pr. of plates Z and P.



1585. *Nitre* was fuzed at 4 and dilute S. A. put into the vessel i. There was no sensible current or decomposition of the Nitre. But on making the fluid in i N. S. acid, the nitre did decompose and there was a current, but not powerful. It must be remembered that at the nitre, as the ends of wires only are used, the decom-

[1] P. 262, par. 1684.

posing surfaces are very small and the current thus much retarded. The decomposition here probably *more* ready than that of S. acid water.
1586. With fuzed chloride of Lead at 4, there was no decomposition or current whilst only dil. S. A. in the vessel i. But when N. S. A. in that vessel i, plenty of action and current—far stronger than with *Nitre*. Remember that here the electrodes have affinity for the elements evolved at both sides, both the chlorine and lead. This facilitate the decomposition and transmission. Current always the same way and dependant upon the zinc and platina in i. The zinc was amalgamated.
1587. Fuzed *chloride of Silver* at 4. This decomposed and the current passed when only S. A. in the vessel i. Hence easiest of all yet tried. When N. S. Acid in i, decomposition very rapid, etc. etc. In both cases, metallic silver reduced beautifully and rapidly upon the surface at the N. Electrode and Chloride of Platina formed at the Pos. Electrode.
1588. We see well in such cases the influence of the chemical affinities concerned at the decomposing place 4 in producing a back effect upon the evolution of the Electricity at the place of generation in i. That is the dependance of one on the other and the direct relation therefore of the powers. Affinity is active at both points, but is as it were connected or related by the current of Electricity in the communicating wires, or in other words *affinity* is *Electricity and vice versa*.
1589. *Iodide of Lead* at 4 was not decomposed by S. A. in i, but was by S. N. A. there. It did not decompose so easily as chloride of lead. Probably because the Iodine has not such power to combine with the P. Electrode as chlorine has. The case came between that of chloride of lead and that of water, using always Platina Electrodes.
1590. *Proto chloride of tin* fuzed at 4 was decomposable whether S. A. or S. N. Acid was in i. But the effect and current was far stronger with the latter than the former. This body nearly as decomposable as chloride of Silver, more so than chloride of lead.
1591. Dilute Mur. acid rendered blue by Indigo was not bleached regularly when S. A. only in the glass i, but was readily when S. N. Acid was used. The current in the latter case was in the

right direction and the bleaching occurred at the *cisode* as it ought to do.

1592. There were some curious appearances when S. A. (diluted) only was used in i. The colour seemed to go, on establishing the circuit, but not at the *cisode*; and the N. Electrode appeared to produce a certain decolouring effect, but quite unlike in character to that when N. S. Acid used.

1593. These various decompositions shew that there is no difficulty in obtaining results with a current of Electricity from a *single pair of plates*.

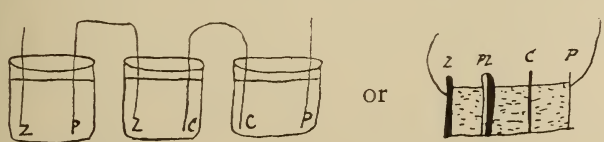
1594. A little N. Acid in i seems to do far more in exalting the intensity of the current than much additional Sul. Acid.

1595. Now experimented with *copper interposed plates*, thus*, dilute Sul. acid being used in the cells. There was plenty of current and also decomposition. The copper did not seem to retard much, if any. When the copper plate was removed and a platina one put for it, then the current nearly stopped, if not quite.

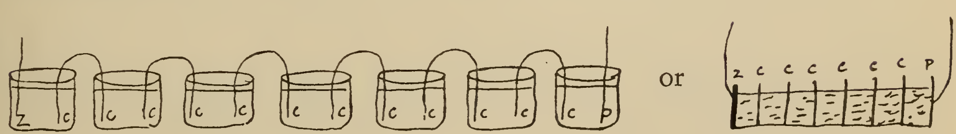
1596†. Now used a series of six intervening copper plates and only one pr. of Platina and Zinc plates, dil. Sulphuric acid being in all the cells. A good current passed. I think there could be no diminution by the coppers. But when only one of the coppers removed and placed by *Platina* the current was stopped. Or when *all* the coppers removed and only the one platina interposed, the current was stopped.

1597‡. Now tried the effect of distance in affecting the power of one interposition of platina in stopping the current from two pr. of Zinc and Platina generating plates. But whether the plates 1 and 2 were six inches apart or whether merely separated by one or two thicknesses of paper (bibulous), still the stopping effect was the same. There was no sensible difference. Hence the result does not depend upon the quantity of intervening decomposing conductor, but on the relation of its elements to the metal.

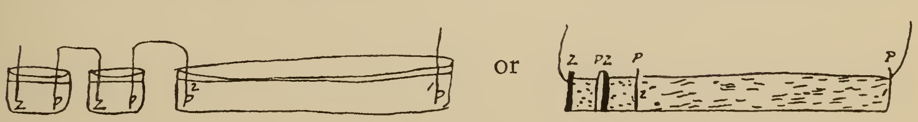
* [1595]



† [1596]

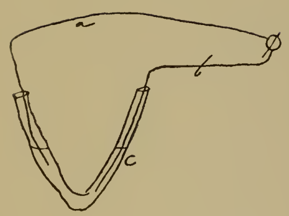


‡ [1597]



1598*. Now tried an experiment or two on the effects of intervening Platina plates, having six pr. of zinc and Platina motors and dil. Sul. Acid in all the cells. With 4 intervening platinas a very feeble current passed. With 3 intervening platinas a current a little stronger passed. With 2 intervening platinas a current still stronger passed, and with one intervening platina a good fair current passed.

1599. But the intensity of the current does not decrease regularly, i.e. alike for each succeeding intervening platina plate. The first interposed does far more than the second, and the second more than the third.

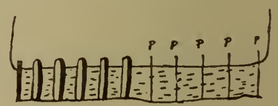


1600. If tin and Platina be made into a thermo-electric circuit, it is very effectual on the application of heat at one point of contact. Thought it likely that when that point was heated so as to cause *combination* with the platina, that at the moment Electricity would be evolved. Made the experiment thus. A bent tube contained tin, a platina wire *a* was melted into one end of this tin and connected at the other with a galvanometer. Another platina wire (thick) was immersed in the other end of the tin and also connected with the galvanometer. When the tube at *c* was heated the galvanometer needle shewed the thermo-electric current. But when the temperature was raised until combination took place at *c*, still no effect on the needle was visible; it remained steady the whole time.

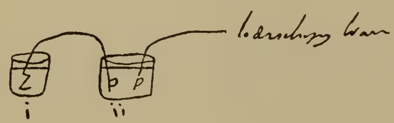
1601. It is perfectly clear from all experiments that Electricity is not evolved during chemical action at the Electromotive plates, but only arranged, i.e. that as much N. Electricity is determined in a circuit one way as there is of P. Electricity determined the other. If the following arrangement† be made in glass vessels, chemical action in i will not evolve electricity, it being insulated. On the contrary, the chemical action is stopped because the current cannot go round, and either metal is just as active in determing Neg. Electricity one way as Pos. Electricity the other way. This latter point important in connection with other views.

* [1598]

or



† [1601]



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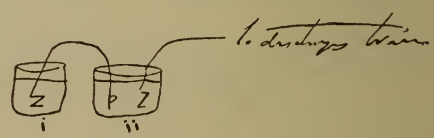
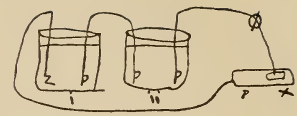


1602. First examined the stopping effects of one interposed platina plate more carefully than before. Thus i and ii contained the same dilute Sul. acid without any N.A. At ϕ was a galvanometer and at X paper moistened in solution of hydriodate of potassa. At first there was, as in former cases, no sensible action (after the first contact) at the Galvanometer. But on waiting a while and then remaking the galvanometer contact, there was an action which however fell instantly as before. This could be repeated continually.



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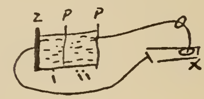


1603. There was also decomposing action shewn by the hydriodate of potassa, this being continual, but more strong, very much, when action was strongest on the galvanometer.

1604. On putting a little N.A. in addition to ii, there was more current both by galvanometer and chemical action, but after a few moments continued contact this increase fell to almost nothing. On intermitting the contact the same renovating effect as that just described was obtained.

1605. On adding still more N.A. to ii, current was at first still stronger, but it diminished as in former cases, though perhaps not to so low a degree.

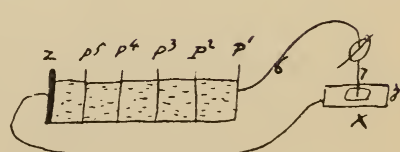
1606. Current of one pr. of motors does pass therefore feebly even though there are two decompositions to be effected, one of water and one of hyd. potassa; and there are also two intervening platinas, one between the two acids and the other between the acid in ii and the hyd. potassa.

1607. If I find by the expts. to be made that the current passes in these cases without decomposing water, it will very much simplify the statements and clear all up. Then I should perhaps find that when no decompositions take place, Sul. Acid, salts, etc. etc. do not render it a better conductor. *Good thought.*

1608*. Now tried stopping power of several interposed platina plates. The acid in all was alike and dilute S.A. (no nitric acid); hydriodate of Potassa on paper at X when required. There was feeble current through all these, shewn both by galvanometer and by decomposition. I cannot help but think that this is the current of an intensity too feeble to decompose water and therefore passing through so slowly. In such a case the number of interposed platina will offer no resistance.

* [1608]

or



1609. Observed here a very curious condition of the apparatus. Suppose that the end of 6 had been separated for a while from contact with P^1; on making the contact there was a feeble current shewn. But if the contact of 6 were made with P^2, the current was stronger still. Or if with P3 still stronger—and with P4 strongest of all, (except P5, which of course inclused then no interpozing Platina plate, and then the current is VIOLENT and continuous).

1610. If, after breaking the contact of 6 so as to suspend the current for a while, it were made at once with P4, the effect was stronger than in any other mode of contact (except with P5 which is always to be avoided); and if afterwards the 6 were brought in contact with P3, P^2 or P^1, no effect resulted, they having been discharged or lowered by the contact with P4 and requiring cessation of the current before they could rise again. It require 5 or 10 minutes to rise again in these cases.

1611. Again, if the end 7 were brought against the platina 8, that caused discharge of the electricity of the apparatus; and if 7 were immediately after placed on the hydriodate of Potassa paper, it did not visibly act, but on allowing time the charge gradually rose and then the passing current was able to decomp. the hydriodate. The same effect happened when the end 6 was against P^2, P3 or P4, and more powerfully. Less time also was then required for renewal of the decomposing effect.

1612. There seems here to be a charge of the apparatus, but if so, it is a very extraordinary charge, for the effect produced on the hydriodate of potassa is equal to two turns or more of the large electric machine, and that electricity could not be stored up in this extraordinary way. Besides which, if it be a charge of the apparatus, then the back discharge which must occur when 6 touches P^2 must be a very odd discharge; and also if the vessel i be taken away, the rest must be able to give a current in the same direction as if that remained. For the power arranged or charged are in *accordance* with and not against the current of the original electromotors.

1613. If when the system is charged the contact is broken at the zinc side as at § in the figure, no current occurs when 6 touches any of the platinas. But still no harm is done, for on renewing

the contact at §, all the effects take place which occur after stopping the current for a while. There is no discharge of P^2, P^3, P^4, etc. etc. by breaking the contact with the zinc like that which is occasioned by making 6 touch P^4.

1614. Agitation of the acid in cup i, or in any or *all* the cups, causes no deficiency of the effect.

1615. Contacts of the metals in the cups ii, iii, iv and v before completing the circuit produces not the slightest alteration in the final effect. *All remains unchanged until the current passes*. The passage of the current only seems able *to let down the state*.

1615½. All the platinas were turned end for end without being changed in their situation one to another. The effect was now much stronger, but still of the same kind and in the same direction, the current also being in the same direction. The platinas had taken the condition of Ritter's plates in fact, and so had diminished the effect I am examining but had not [been] able to overcome it. But the cause of the effect MUST BE *in glass i*.

1616. Now experimented with only one intervening platina and after a while discovered the cause of the whole: it depends entirely upon the nature of the bodies in contact with the zinc plate. When the current passes at the first moment of contact water is decompozed against the zinc oxide formed, and this neutralizes the acid immediately in contact with it; so that instead of being in contact with acid, it is rather with a solution of sulphate of zinc. Hence the plate cannot oxidize and the current sinks. But on breaking contact for a while (which would keep the zinc in that state) the surrounding fluid gradually mingles with and renews the first state of things, and then the current can be reformed and so on. All the effects are thus explained.

1617. This result shews the importance attaching to a consideration of the change in the nature of the substances formed in contact with the plate. The rests will be exceedingly valuable in a battery of amalgamated zinc plates, etc. etc. etc.

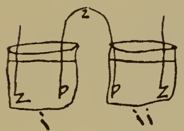
1618. The successive forces at P^5, P^4, P^3, etc. etc. seems to shew that there is retardation by them. It may be only what would happen from elongating the series, but if it depends upon the successive interruptions and not upon mere increase of distance in cases where water *not decomposed*, then must consider their

extent and the nature of the interference; for if water is not decomposed, then I see no reason (theoretical) at present why electricity should pass with more difficulty between *water and platina* than between *water and water*. But it may be so.

1619. Now experimented upon decomposition of different bodies by a single pair of plates, using platina surfaces for decomposition and having therefore metallic contact. A solution of sulphate of Soda in a folded piece of litmus and turmeric paper at X gave acid at the end of P^1 and alkali at the corresponding surface of P^2, when the fluid in the vessel i was N. Sul. Acid, but not when it was dil. Sul. acid only.

1620. Nitrate of Silver on paper was readily reduced at X by N. S. Acid in the vessel i. Silver appeared beautifully.

1621. Acetate of lead did not seem to be reduced.



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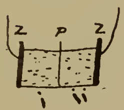


1622. Now experimented contrasting different fluids together, thus. No. i contained S. A. s.g. 1·300; No. ii acid consisting of one part of that s.g. 1·300 and four of water, and therefore very much weaker. In this case, whether tried by galvanometer or chemical action, the motive power was as nearly balanced as could be; on the whole the weaker acid predominated. Great care was taken and was needful in changing the zincs in the two vessels one for the other, and in turning 2 round so as to balance errors due to variation in them. The weaker cell was very little but still decidedly superior in current to the stronger cell.

1623. On putting a little N. A. into ii, or the weaker cell as to acid, it was able still more decidedly to overcome the cell No. i in power.

1624. Tried two weak and two strong cells against each other, thus*. i and ii contained strong S. A., i.e. 1·300; iii and iv only weak S. A., about one of the former to 5 of water. The strong were quite neutralized by the weak and on the whole the weaker had a very little advantage. The plates were changed about in every direction so as to avoid the influence of differences between them.

1625. On arranging one pair with Stronger S. A. against two pair with weaker S. A., i.e. with i strong S. A. whilst ii and iii had weak S. A., the latter two had the advantage. But there was great obstruction to the current. Still there was more than with

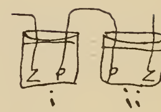
* [1624]  or 

one pair only of weak S. A. opposed to one pr. of strong S. A., but there was nothing like the effect of one pr. of weak S. A. alone and unembarrassed by other.

1626. Again, on opposing one pair with weak S. A. to two pair with strong S. A., there was the same effect, i.e. a current in the direction governed by the two with strong acid, but not equal to what might be expected from two less one. It was not a tenth part of the effect due to a single pair of unembarrassed plates in Strong S. A.

1627*. Now reversed one pair out of four, the acid in all the cells being equal in strength. Very little current of Electricity passed; the one pair reversed took greatly from the other three pair, and as to practical results almost neutralized them.

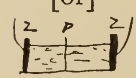
1628. Now compared amalgamated and unamalgamated zinc against each other in the same Sulphuric acid, and took care to change them about so as to neutralize the irregularities in other things. The amalgamated zinc always had the advantage over the unamalgamated zinc—notwithstanding that the action upon the unamalgamated was very strong, much hydrogen being evolved, and the action on the amalgamated very little. The reason is a good one. Because the amalgamated zinc is less acted upon, there is less sulphate of zinc against it and in that respect it is superior and retains its superiority, though lowered by the presence of the mercury on its surface.



or

1629. Now tried strong and weak Mur. acid against each other in two cups or cells with zinc and Platina plates. No current was produced, so that their powers seemed to be equal. A little nitric acid added to the weaker made it predominate in force slightly; a little in the stronger made it predominate still more.

[or]



1630. But there is a very great quelling effect where plates are opposed to each other, for they act like platina plates in obstructing even before they can come into play as electromotors to produce a counter current.

1631. Dilute Muriatic and Sulphuric acid opposed to each other appeared to be very equal in motive force. In both there is hydrogen to be separated and so far they are alike.

1632. Strong Nitric acid has a superiority over weak nitric acid, but not much.

* [1627]  or 

1633. Strong Nitric acid against pretty strong sulphuric acid has a superiority, but not much. More alike than one might have supposed.

1634. Must in all these cases remember that the opposing plates, even if not Z and P but all Platina, would retard much; and that being Z and P, it doubles the resistance, etc. etc.

1635. Remember also that these indications are by the Galvanometer, which is not a measure of intensity but of quantity, and that if tried by a series of compounds decomposable at different degrees and so forming a graduation, that other results might come out. Nevertheless the results are very useful practically, although they do not give pure and simple effects from which the true laws may be deduced.

1636. When metals are the Electromotors, is not the evolving action always between the metal and the element oxygen or chlorine with which it combines. In that case the after combination of the oxide formed with the acid is of no avail and produces no additional result of an electrical nature; but rather in an incidental manner retards the action by investing the plate with an unfavourable fluid.

1637. With regard to the need of intensity in acting upon bad conductor or upon a greater length of the same conductor. There are good reasons why a greater length should require a higher intensity, for as the present state is sustained by a certain natural exertion and condition of the chemical affinity which it requires even an extra force to overcome for a single particle, so where there are many particles, the resistance to the change must be greater, being equal to the sum of the resistances of each of the particles subject to the variation caused by the variation in amount of the resistance at the *end* of the chain of particles, where the last particle has either to be actually excluded or combined with a body having more or less affinity for it.

1638. Must consider the Volta electric motion of elements and their final places of rest or evolution in a circle and in relation to the direction of the similar elements in the motive arrangement, for it is they which determine the courses of the other elements. Thus arranging them in a circle, if oxygen is the motor by combination with a metal, then in the circle other oxygen or chlorine

go elsewhere in the same direction. Or if chlorine the motor, elements like it march under its guidance.

1639. Observe how this will bear in Sulphur or other combinations producing volta electricity.

1640. Particles of an electrolytical body are all mutually connected and dependant—are in relation with each other throughout their whole extent in the direction of the current. Hence, if the last not disposed of, the first is not at liberty to take up its place in the new combination, which the superior affinity of the Electromotor tends to produce.

1641. Must make out what happens in cases of chemical action with no current.

1642. We seem to have the power of deciding, in certain cases of chemical affinity (as of zinc with the oxygen of water, etc. etc.), which of two modes of action of the *one power* shall be exerted. In the one mode we can transfer the power on, it being able to produce elsewhere its equivalent of action; in the other it is not transferred on but exerted at the spot. The first is the case of Voltaic Electric production, the other the ordinary cases of chemical affinity. But both are chemical actions and due to one power or principle.

1643. That no electricity is set free in the latter case shews the equality of forces and therefore of Electricity in those quantities which are called Chemical equivalents. Hence another proof that Chem. affinity and Electricity the same.

1644. In Becquerel's expts. on flame and other cases of chemical action, the effects were probably only incidental and perhaps so irregular, from our ignorance of knowing how to obtain the true effect, as to be almost ACCIDENTAL. The results are in *no proportion* to the quantity of Electricity *really concerned* in the phenomena.

1645. I must very closely consider and examine a case of combination in which no electric current produced. Such as Zinc in dilute Sulphuric acid or oxide of lead in Nitric acid, etc. etc. etc. What become here of all the electricity which must pass during the combination? How is it discharged between the particles? Of course they are able to neutralize each other, but how do they neutralize?

1646. Is not rubbed glass and the rubber exactly in the state of

Zinc and the oxygen of water in an electromotive circle, i.e. when the rubbed glass and the rubber are separated are they not in the state assumed by the zinc and the oxygen before they combine and before the contact is made in a single voltaic circle. They probably give an exalted view of the conditions of the particles of the zinc and oxygen, a permanent view as it were. How do the states agree?

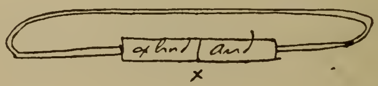
1647. Would not this view, if supported, reduce both modes of evolution to one common principle. The mutual influence of neighbouring particles—in the glass not proceeding to a full effect: in the voltaic circle being completed and being followed in succession by a multitude of others of the same kind. In the last it is the attraction of the Zinc for the oxygen of the oxide, and this would tell in well for the instances of induction, etc. perhaps, of common electricity.

1648*. What would result if, in place of evolving electricity by the union, etc. of Elementary bodies, as Metals with oxygen, chlorine, etc. we could arrange an acid and a base? Oxide of lead would do for a base and sul. acid perhaps for an acid. Boracic acid would not do, but observe we should have no electricity, for oxide of lead is no conductor except it decompose, and it would not decompose in such a case. There would be no transmission therefore in such a case because the combining bodies are not conductors. In all voltaic circuits the combining body is either a conductor per se, or being a zetode, it constitutes an electrolytical conductor with its companion element.

1649. Therefore cannot expect electric currents from combination of acids and alkalies.

1650†. But if the acid could be a hydracid, then the course of the elements would allow a current; but the affinities would probably be against it. Thus let *a b* be platina, *c* oxide of lead and *d* Mur. acid (if it were possible), the oxide being of course in the fluid state, for then only is it electrolytical. Then if the affinity of the lead of *c* for the chlorine of *d* were very great, they might combine at *e*. Oxygen would travel in *c* towards *a* and be evolved there; hydrogen would travel in *d* towards *b* and be evolved there, and an electric current might run round. Or if *c* could be a fluid oxide of a metal and *d* water, the same might happen. But no

* [1648]



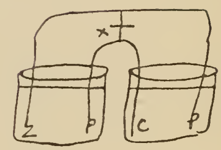
† [1650]

combination of Elements or of compounds in the free state can produce an electric current unless they be conductors like the metals and not Electrolytically. Now we have none such and have no right therefore to expect such currents.

1651. Hence more reasons why Becquerel's results only accidental.

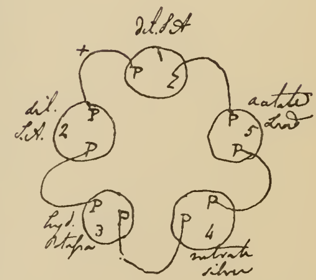
1652. The circumstances in fact do not exist with acids and bases or with independant bodies, either elementary or not (except the metals), which are necessary to the construction of the pile. Hence the uses and relations of metals, as zinc, etc. Hence all the circumstances and conditions limiting the construction of the voltaic pile.

1653. As to power of comparing intensity, I wish I had a true measurer of it. What would this do, using a substance for decomposition at X? Still this not good; and should probably obtain different indications at X by using a galvanometer there or different chemical decompositions.



25 FEBY. 1834.

1654. As to intensity of a current and its power of effecting decompositions, etc. etc. As the current of one pair of plates was found in the expts. of the 22nd instant to pass through four intervening platina, I arranged an experiment last night at 7 o'clk. in the following manner. Five glasses were put in a circle so to bear connecting by the platina plates, etc. and charged with different solutions. No. 1 had the dilute Sul. acid, s.g. 1·300 about; No. 2 the same acid; No. 3 a solution of hydriodate of potassa; No. 4 a solution of nitrate of silver, and No. 5 a solution of acetate of lead. These were then connected by pairs of metal plates, only one pair of Z. and P. being uzed; the rest being pairs of platina only. The cup No. 1 contained the Zinc and platina pair, which acted as Electro motors to the whole.



1655. This was left all night, and at 8 o'clk. A.M. to-day could observe no traces of decomposition anywhere.

1656. At 12 h. 30′ A.M. to-day examined the whole carefully. First took out the pair of platina plates X, introduced two separate plates of platina and tried between them at X for a current of Electricity by sol. of hydriodate of potassa on paper, so as to be

sure that one had passed during the night. Found that there *was a current passing*, although it was very weak.
1657. Then looked closely at the several cups. In No. 1 Gas was in act of evolving and rising from the Zinc by the direct action of the platina. But there was *none on the platina*, as I expect there would have been had a *decomposing current* existed. A glass rod having been introduced to keep the Zinc and platina from touching each other, *that* had many bubbles on it as if it had acted as a nucleus in evolving air from the acid; but if so, such air was probably hydrogen evolved and dissolved at the zinc surface. This solution of hydrogen curious, and an effect that would prevent appearance of gas on Neg. Electrodes even when decomposition (feeble) going on.

No. 2 vessel. *S. Acid.* No gas either on the Pos. or Neg. Electrode in this vessel, i.e. no bubbles adhering and no appearance as if any had risen from them.

No. 3 vessel. *Hydriodate of Potash* solution. No coloration here as if any decomposition. No gas on either Electrode.

No. 4 vessel. *Nit. Silver.* No gas on either Electrode. No appearance of Silver on the N. Electrode.

No. 5. *Acetate of Lead.* No gas on either electrode. No lead on N. Electrode.

1658. Even if hydrogen evolved in 1, 2 and 3 at N. Electrode and dissolved, still here and in 4 it should have reduced metal. Hence strong reason for concluding that water, etc. had conducted a current during the night, but *had not been decomposed* by it.

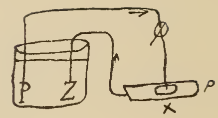
1659. Connected the platinas in the cups 3 and 5 and finally also in 4. Then had a very fair current when examined by hydriodate of potassa at X. Of course it soon fell in power, according to what is said in the expts. of the 22nd, but still a current passed. Left things in this state at 20′ to 1 o'clk. P.M.

1660. Re-examined them at 11 o'clk. on the 26th, i.e. next day. There was gas on the Zinc in the first glass, and also on the glass rod and the platina plate in the same glass. But there was no gas on either of the Platina plates in the second glass and so far no apparent decomposition by the current.

1661. On proceeding to test for the current at X, found by hy-

driodate of potassa that it *was passing*, but in a very feeble state. Being counteracted by the peculiar Marianini state of the plates in the second glass.

1662. Experimented on the current formed by solution of common salt in cup with Zinc and Platina plates, thus. When paper moistened in hydriodate of potassa was put at X, there was decomposition instantly, Iodine being evolved at the end of the wire.



1663*. Then arranged the apparatus in the following order, so as to have two decomposing surfaces at 1 and 2, and by making the ends of the wire 3 touch either the plates of platina or the papers moistened in the solutions, I could arrange so as to have only one decomposition at a time, or both or neither at pleasure.

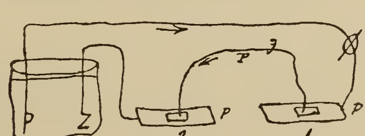
1664. Put hydriodate of potash at 2 and Nitrate of silver at 1 and connected so that the current would have to pass through both substances. After a while there was decomposition of the iodide, but none of the nitrate of silver. Hence there seems to be a good distinction as to the necessary intensity, for the current passed through both and was sufficient in intensity for the iodide but not for the nitrate.

1665. As the nitrate an acid salt, perhaps the hydrogen may have been evolved but was occupied in decomposing nitric acid. Still I do not think this likely, but can try with the sulphate or the acetate of silver.

1666. Now put turmeric paper moistened in solution of sulphate of soda at 1, and hydriodate of potassa on white paper at 2. When both substances intervened in the course of the current, there was a slow effect on the iodide but none on the sulphate of soda. When the iodide alone intervened, then there was quick decomposition of it. When the sulphate of Soda alone intervened, then could get no signs of decomposition. Here again a distinct difference; the intensity enough for one but not for the other.

1667. As to the *evolution* of Electricity, there was plenty of action on the zinc and oxidation in the solution of salt and oxide of zinc falling to the bottom. It is well worth while ascertaining whether any zinc *remains* in solution, for if not, the case is an excellent one to shew that the oxidation or chloridation of the metal is the

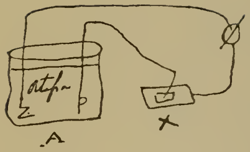
* [1663]



source of electricity and not the solution of the oxide in an acid. The case probably a very good one to shew decomposition of water by zinc without interference of further chemical action by the oxide formed.

1668. Ascertain also whether if zinc and platina in contact in solution of salt, hydrogen will be evolved against the platina. May seal up hermetically a zinc and platina pair or may close it up by mercury. The oxide separates in an insoluble flocculent form.

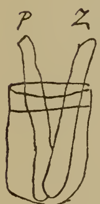
1669. I put litmus paper dipped in sul. soda at X of the first brine arrangement, placed the whole under a wet bell glass to prevent evaporation and left it all night to see if then any decomposition of the Sul. Soda would occur; but there was no appearance of it. Yet the current had been passing, and on substituting hydriodate of potassa for the sulphate of soda there was decomposition immediately. Good expt.



1670. Experimented now with Alkali in place of acid, for if my present notions right, Alkali ought to give the same effects and the same direction of current as acid, i.e. if the oxidation of the metal is the real cause of the electric current. When solution of Potassa was in the vessel A, there was a strong electric current when no decomposition at X intervened, almost as strong as with acid, and the direction of the current *was the* SAME.

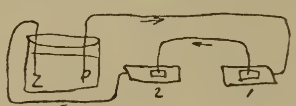
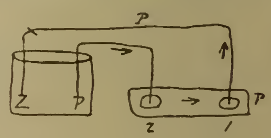
1671. The current of the single pair in the Potassa was able to decompose hydriodate of potassa at X freely and powerfully; also nitrate of silver; and even Sulphate of soda, giving acid at the cisode and alkali at the exode of one decomposing portion.

1672. Then arranged the apparatus as before, so as to give two decomposing places, thus*: but was scarcely able to effect the simultaneous decomposition of sul. soda at both places. Still it did occur feebly. Hence this a very powerful agent. More powerful than Sulphuric acid apparently.



1673. On making Zinc and Platina in the same glass of caustic potassa touch each other, there was strong evolution of hydrogen on the platina, just as if they had been immersed in dilute Sul. Acid. It was amalgamated zinc which was used in all these experiments.

1674. Then used a strong solution of Ammonia in place of the

* [1672]  or thus 

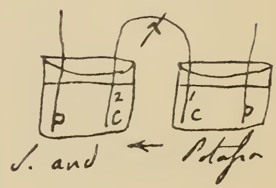
caustic potash solution. There was a powerful current in the same direction as with acid or potash but not quite so strong as the latter. It however decomposed Iodide of Potassium and Nitrate of silver and feebly also Sulphate of Soda.

1675. On making the solution of ammonia weaker it acted more powerfully, but in the same direction.

1676. As pure ammonia solution is a bad conductor I added some Sulphate of ammonia to it, and now it acted very well, approximately to solution of potash; but direction of the current always the same.

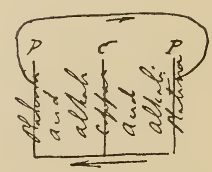
1677*. Now opposed two cups with the same motors, but one containing the *dilute S.A.* 1·300 and the other strong solution of *potash*, in two ways. At first there was an effect in favour of the Alkali?, but in a few seconds it settled to nothing and there was no permanent current either by the galvanometer or chem. action.

1678. Made the same experiment, using copper instead of zinc. Here there was a little effect at first, but it immediately fell to nothing and no permanent current was produced. The effect was in favour of the alkali?, i.e. both the motors causing currents from the copper through the liquids to the platina; that in the alkali cup was apparently the strongest or produced the visible effect.



1679. On taking out the copper bridge *x*, which was a long slip or plate, and turning it round so that the end 1 went into the acid cup and the end 2 into the alkali cup, there was an effect produced at the galvanometer of a current in the direction of the arrows. It lasted only an instant and was renewed if the copper were again taken out and turned half round, and so on continually.

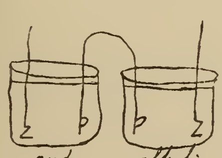
1680. There was of course action between the acid and alkali each time this reversion of the copper was made, for the ends of the metal took acid into the alkali cup and alkali into the acid cup, and the order of bodies at the first moment of immersion would be thus. Both pairs of acid and alkali therefore are in such order as to aid each other in any electrical action due to their mutual combination. See page 352[1], etc.



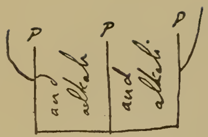
1681. Now used platina instead of copper for the intervening plate. Still the effect was produced upon inversion as before, but

[1] i.e. see par. 1719 *et seq.*

* [1677]



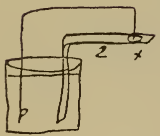
or thus



not so strongly; yet always in the same direction. This however seems to prove that it is the acid and alkali which by combining produce the current. The electricity produced is however, I think, a very small portion of that which would be produced if the whole of that evolved by the acid and alkali each time combined could be directed. It is perhaps only that directly touching the plates which causes the current.

1682. But it shews that acid and a base combining on the surface of a plate, as when zinc is oxidized by dilute S.A., may produce electricity; and that the water does not do all?

1683. On comparing the effect when the intervening plate was platina or copper, it was decidedly greater with the latter. Hence it would appear that the copper itself is so prepared by the previous contacts with the acid and alkali as to be assisting in producing a current like that due to the acid and alkali alone. In that case, the limb of the copper which has been in contact with the acid, and is at the moment in contact with the potash, is negative to the other, determining a current to itself from the fluid.



1684. Repeated the expt. of 19th Feb. p. 320[1], but could not obtain the intermitting action on the hyd. potassa at X. It always decomposed there and was a very regular good case of decomposition without metallic contact.

1685. On removing the glass and using others containing either Potash or ammonia, the same effect was produced and in the same direction. Very good. On touching the zinc with the Platina then the order of the current reversed.

1686. In these cases NO ELECTRICITY can be produced or result from union of oxide formed with an acid. GOOD.

1687. If a certain intensity be required for a certain decomposition, as of water, and if when a certain intensity at the motor is produced it can decompose without relation to quantity, as seems to be the case, *then* we may arrange things so that the *same quantity* of electricity may pass in the *same time* in at the *same surface* into the *same decomposing body* in the *same state*, and yet differ in intensity, *decomposing in one case and in the other not.* For taking a source of too low an intensity to decompose and ascertaining the quantity passed in a given time, it is easy to take

[1] i.e. par. 1578.

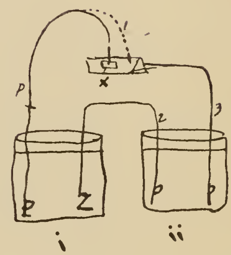
a source having an intensity sufficient and reducing its quantity to the same proportion as the last by introducing bad conductors.

1688. But if intensity depends upon the intens[ity] of action or chem. affinity at the first source and not upon the quantity, then intensity OF THAT CHARACTER cannot be lowered by enlarging the surfaces of the electrodes or admitting more easy discharge of the current, and then the analogy *would fail* with ordinary electricity.

1689. Can all bodies conduct a little, i.e. more or less at intensities below the *electrolytic* point?

1690. By a scale of motors of different kinds and intensities, obtain a gradation of powers, and by them obtain a gradation of decomposition. Also evidence that a certain intensity is required.

26 FEBY. 1834.



1691. Put on an experiment with two glasses of acid S.A., and alike, thus. When tried as in the figure there was good decomposition of the hyd. potassa at X. The wire 1 was then put into the dotted position so as to complete the metallic communication there, and all left for several Hours: this was at about 11 o'clk.

1692. At about 5 o'clk. replaced the wire 1 on the paper at X and found a current, but only a feeble one; not at all like what ought to be produced by the arrangement in the vessel i, provided no new state had been taken up at ii. But in fact the plates in ii had, by the passing of the current, assumed the Marianini state so powerfully as almost to neutralize the current and prevent passage. This was proved thus. The wire 1 was lifted up and insulated. A platina wire was made to touch at 2 and 3, connecting the plates in ii, and as I know from former experiments, discharging them and getting for the moment a counter current. Then taking away that wire and letting 1 down upon the platina first, if I pleased, and then upon the hyd. potassa at X, there was much more powerful decomposition shewing a much freer current. On connecting 1 with the Platina plate as by the dotted line and leaving it there, the plates in ii gradually resumed their former counteracting state and again reduced the current through the whole almost to nothing.

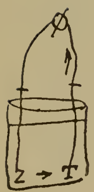
1693. This condition of the plates very curious. If not due to

reduction of Elements and fixation upon them, is extraordinary. Can it be hydrogenation and oxygenation of the Plates? Silver, copper, etc. etc. seem in the [illegible] capable of this kind of state on surface, etc. etc.

1694. If so, might prepare them in other ways, as in oxy water, etc. And also let them down other ways, as by hydro compds., etc. After which, contact should give no current. But all this must wait.

1695. I left the apparatus in the connected position designated by the dotted line to be examined as to gas or not in ii tomorrow. See (1728) 8 March 1834.

1696. Now made a general set of Experiments on two metals immersed in the same cup, containing either S.A. 1330 or solution of caustic potassa or caustic ammonia, the latter rendered a good conductor by the addition of Sulphate of ammonia. It required very great care to have perfectly clean metals each time. Not merely was it needful to remove any of the solution adhering to a metal from previous immersion, but even to remove any oxide adhering to it. Wiping or cleaning of tin, for instance, by a cloth, would not do; it required cleaning by sand paper; and again, the same piece of sand paper could not be used for two different metals, or even twice in the same place for the same metal. The form of expt. was thus, and the direction of the current is always given as from one metal through the liquid conductor to the other. Thus from Zinc to the Tin means from the zinc to the solution and from it to the Tin; back again through the galvanometer to the Zinc.



1697. Metals used, *Zinc and Tin.*

Z & T	In ammonia.	Current from Z to T,		but very feeble and transient.
	Potash.	Do.	Do.	stronger, but transient.
	S.A. 1330 about.	Do.	Do.	not strong for acid, but still stronger than either of the former.

1698. Metals used, *Zinc and Lead.*

Z & L	In Ammonia.	Current from Z to L,		feeble and transient.
	Potash.	Do.	Do.	strong, as in acid.
	S.A. 1330.	Do.	Do.	strong.

1699. Metals used, *Zinc and Iron*.

Z & I	In Ammonia.	Current from Z to I,		powerful.
	Potash.	Do.	Do.	powerful.
	S. A.	Do.	Do.	powerful.

1700. Metals used, *Zinc and Copper*.

Z & C	In Ammonia.	Current from Z to C,		very strong.
	Potash.	Do.	Do.	very strong.
	S. A.	Do.	Do.	exceeding strong.

1701. Metals used, *Zinc and Silver*.

Z & S	In Ammonia.	Current from Z to S,		moderate.
	Potash.	Do.	Do.	moderate.
	S. A.	Do.	Do.	exceeding strong.

1702. The difference here between Copper and Silver in the Alkalies remarkable.

Now *Tin* with the other metals.

1703. Metals used, *Tin and Lead*.

T & L	In Ammonia.	Scarcely any current, not decided.
	Potash.	Current from T to L, strong.
	S. A.	Current from L to T, very feeble.

A remarkable case this of inversion and superior effect of alkali.

1704. Metals used, *T and Iron*.

T & I	In Ammonia.	No sensible current.		
	Potash.	Current from T to I and very fair in strength.		
	S. A.	Do.	Do.	a little stronger.

1705. Metals used, *T and Copper*.

T & C	In Ammonia.	Current from T to C,		moderate, transient.
	Potash	Do.	Do.	same same
	S. A.	Do.	Do.	moderate, but stronger than other two.

1706. Metals used, *Tin and Silver*.

T & S	In Ammonia.	Current from T to S,		very feeble.	
	Potash.	Do.	Do.	stronger.	temporary.
	S. A.	Do.	Do.	same	

1707. Metals used, *Tin and Platina*.

T & P	In Ammonia.
	Potash.
	S. A.

Now combination of Lead with the succeeding metals.

1708. Metals used, *Lead and Iron.*

L & I	In Ammonia.	Current from	L to I,	very feeble.
	Potash.	Do.	Do.	stronger.
	S. A.	Do.	Do.	still stronger.

1709. Metals used, *Lead and Copper.*

L & C	In Ammonia.	Current from	L to C,	moderate.	transient.
	Potash.	Do.	Do.	moderate.	
	S. A.	Do.	Do.	strong at first, but instantly over.	

On cleaning the lead again is renewed, etc. So much for investing coat formed; it insulates, etc. etc. etc.

1710. Metals used, *Lead and silver.*

L & S	In Ammonia.	Current from	L to S,	feeble.
	Potash.	Do.	Do.	stronger but temporary.
	S. A.	Do.	Do.	as last, temporary.

1711. Metals used, *Lead and Platina.*

L & P	In Ammonia.
	Potash.
	S. A.

1712. Now Iron. Metals Used, *Iron and Copper.*

I & C	In Ammonia.	Scarcely a current,		very doubtful.
	Potash.	Current from	I to C,	very feeble, almost doubtful.
	S. A.	Do.	Do.	strong.

1713. Compare Iron to Zinc. S. A. can act on oxides of both and effects are alike; but the Potash can dissolve oxide of one and not of other, and observe the consequence.

1714. Metals used, *Iron and Silver.*

I & S	In Ammonia.	Current from	I to S,	scarcely sensible.
	Potash	Do.	Do.	weak.
	S. A.	Do.	Do.	not very strong.

1715. Metals used, *Iron and Platina.*

I & P	In Ammonia.	Current from	I to P,	very feeble.
	Potash.	Do.	Do.	very feeble.
	S. A.	Do.	Do.	very strong.

1716. Metals used, *Copper and Silver.*

C & S	In Ammonia.	Scarcely a current,		doubtful.
	Potash.	Current from	C to S,	very feeble.
	S. A.	Do.	Do.	very feeble.

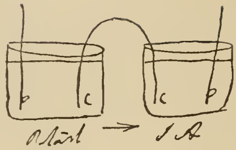
1717. Metals used, *Copper and Platina.*

C & P { In Ammonia.
Potash.
S. A.

1718. Metals used, *Silver and Platina.*

S & P { In Ammonia.
Potash.
S. A.

1719. Repeated the expt. of turning the copper (page 344[1]). The current was very feeble each time, but was from the alkali to the acid each time. It fell to nothing each time. On using Ammonia in place of potash, still the current was from the Alkali to the acid, though very feeble.



1720. On using Platina in place of $\overset{\frown}{c\ \ c}$, there was very little current, and sometimes one way, sometimes the other. It is quite evident from these and the expts. with tarnished Plates that the investing coat of matter can determine a current, but it lasts only a short time and seems to be quite overcome and swallowed up by that which the combination of oxygen, chlorine or other elementary matters with the metals causes.

MARCH 5TH, 1834.

1721. *Reference.* Becquerel on the interruption or non-interruption of a feeble current, according as the metals dipping in a solution had attraction for substances present or not. See Annales de Chimie, xxxv, or Quarterly Jounl. of Science, xxiv, p. 462. Relates to the reason of its passing in some cases and not in others, for is to me now evident.

1722. *On Electro chemical theory of combination.* Quar. Journal of Science, xxvi, p. 428 or . Fechner.

1723. *On Electro-chemical theory of pile.* Ritchie. Phil. Trans. 1829.

1724. *Ignition of wire by Voltaic Electricity.* Becquerel. Quar. Jounl., xxvii, 189.

1725. *Conductibility conferred on water, etc.* De la Rive. Do., xxvii, 407. Also ii, p. 465; or Bib. Univ., xl, 205.

1726. Brande on *decomposition of Vegeto Alkalies.* Quar. Jour. 1831, i, p. 250.

[1] See par. 1679.

1727. Turner's last Elements, pp. 98, 126.

1728. It is now ten days since the apparatus (1695) was put on, yet there is a current, and a good one, through the iodide of Pm. to decompose it; yet no appearance of gas in the second vessel. Hence I really believe that the current passes, but the intensity is *not sufficient* to cause decomposition of water. 10th March, Do.

21 MARCH 1834.

1729. Experimented as to spark with single pair of plates on making and breaking contact. The plates were a zinc hollow cylinder and a double hollow cylinder of copper, immersed in a jar of Sul. Nitric acid. The connection was completed by a copper wire, amalgamated at the ends, dipping into two cups containing mercury and attached to the zinc and copper plates.

1730. I found that there *was a spark* on making contact as well as on breaking it, if the metal surfaces were clean and dry; and that on the whole it was more brilliant than that occurring on breaking contact.

1731. When a film of oxide was present, it interfered much with the spark, which then was often *feeble* on making and sometimes failed. The breaking spark was in such cases most frequent and most brilliant.

1732. On putting a stratum of water over the mercury, the spark much diminished in brilliancy, for now no mercury burned; but it was very regular both on *making* and *breaking* the contact.

1733. On using a platina wire instead of amalgamated copper wire, the spark was much diminished in brilliancy, but happened both ways; every now and then failing both ways.

1734. On making the contact between Platina on both sides, then spark very small, but in both directions. On making and on breaking contact. Also under water.

1735. On making the contact between the platina wire of the copper plate and the edge of the Zinc plate, there was the spark both on making and breaking contact.

1736. The true spark is very small; that obtained by using mercury surfaces is due to the combustion of the mercury. The circumstances favouring the burning of the mercury are more favourable on breaking than making contact, for the act of

breaking exposes clean surfaces of mercury, but that of making does not; and hence it has been supposed the spark only took place at the breaking and not on making contact.

1737. Shall repeat with clean Copper and Zinc plates and amalgamate the latter so as to obtain the simplest form of expt.

19TH APRIL.

1738. To ascertain whether, when prot. iodide of tin is electrolyzed, per iodide of tin or iodine is evolved at the cisode. The prot. iodide is fixed in close vessels, but in the air readily burns into per iodide and oxide of tin, even with flame. Hence could not act in open vessels and in close vessels is very difficult. But I obtained sufficient evidence to shew me that as with the chloride, so with the iodide of tin. A per iodide is formed at the cisode, which flies off, and there is little or no action on the positive electrode.

1739. Is fuzed anhydrous acetate of Soda an Electrolyte? Yes, and I believe the proximate elements, acetic acid and Soda, first separate. But these are also reacted upon, and at the N. Electrode carbon, Sodiuretted hydrogen, etc. separate; and at the P. Electrode a little gas, but I could not separate it to make out accurately its nature.

1740. As to whether the length of a wire altered the effect of a *given quantity* of electricity in rendering any part or all parts red hot, found it did not. Thus a battery of four troughs was connected with a volta electrometer, using in the circuit a piece of fine platina wire; and this was shortened gradually as the power of the battery failed, so that the wire should always be dull red hot, as seen on a back ground of black paper. The current was continued for two minutes and the length of wire sustained red hot was from 8 to about 4 inches. The quantity of gas evolved was then noted.

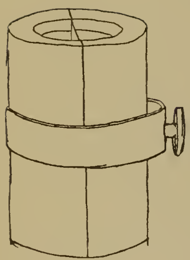
1741. The experiment was resumed with the same wire (always in a horizontal position), using only two troughs. The length of wire now preserved red hot was from $3\frac{1}{2}$ inches to as little as $\frac{3}{4}$ of an inch only. This being continued for two minutes, exactly the same quantity of gas as in the former case was evolved. The experiments were repeated with the same result.

1742. Thus it is proved that the ignition of a platina wire has no relation to its length but only to its diameter. For the same quantity of electricity that will heat one inch of wire red hot will heat a hundred inches if it pass through. The only effect of a long wire is to retard the current and so diminish the quantity which passes, and hence the lower heat in the usual form of making the experiment.

1743. The first effect of a battery is not an effect of accumulation, but only a result of the favourable state of the fluid at the plates. It is both: Oct. 1834.

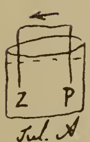
13 MAY 1834.

1744. Have begun experiments with single circles to determine the current induced by the use of different metals and especially different fluids, for I do not believe that metals are rendered, as the phrase goes, positive by acids and negative by alkalies.

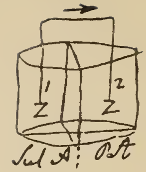


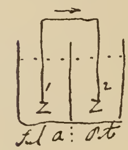
1745. Have had a wooden cup made, cylindrical in form and open only at one end. This had been cut down the middle into two halves, which could be held together so as to make one vessel by a ring going round it which could be tightened by a screw. If before putting the two halves together a piece of filtering paper were interposed, and then the screw tightened, the paper divided the cup into two cells or portions, into one of which acid and into the other solution of alkali could be put; and these were then in electrolytic communication and could be used to form the fluid communication between two pieces of metal, either of the same or different metals, and which might either communicate together directly or through the wire of a galvanometer and yet form only a single voltaic circle.

1746. This cup is an excellent general instrument of investigation. Can try all sorts of combinations by it; it becomes a universal chemelectric test. It will supply the arrangement of *two metals and one fluid*; or of *two fluids and one metal*; or of *two metals and two fluids*; or of *any combination* of these in the simplest manner.



1747. I shall represent the results of experiments diagrammatically thus: the first figure shews that when zinc and platina are in the same portion of dilute Sulphuric acid, the electric current by the Galvanometer is from the P to the Z through the metal and from





the Z to the P through the liquid. Fig. 2 shews that when two pieces of Zinc are put into dilute S.A. and sol. of potassa in electrolytic communication, the current is from Z^1 to Z^2 through the metal and from Z^2 to the Alkali, then to the acid, and then to Z^1 below. That in fact that Zinc is rendered more positive, as the phrase is, by solution of potash than by dilute Sulc. acid.

1748. *Zinc in dilute S.A. and in Potassa.* The current was always as in the figure, so that the Zinc in alkali was positive to the Zinc in acid, and strongly so at first—so as to swing the galvanometer needle quite round. The effect was strongest at first. As effervescence at Z^1 from direct action of the acid came on, the deflection diminished, but was renewed by taking out Z^1 and after a moment returning it, touching the surface of the acid with it. In this way the effect was very strong.

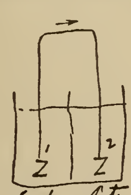
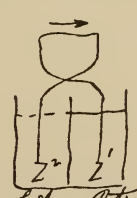
1749. By keeping Z^2 in the Alkali and making Z^1 touch the surface of the acid at intervals of about $\frac{1}{2}$ or $\frac{1}{3}$ of a second, a far greater deflection of the needle could be preserved than if the pieces of metal were retained constantly in the solutions.

1750. The pieces of zinc and of other metals used in these experiments to touch the acid and alkali were merely wires about the $\frac{1}{15}$ or $\frac{1}{20}$ of an inch in diameter and made perfectly clean.

1751. A little piece of platina wire was twisted round the end of the wire Z^2, or that in the alkali, to diminish its force a little or render it less positive towards Z^1, but still, though this platina was with Z^2 immersed in the alkali, it made no difference in the direction of the current nor apparently in its force.

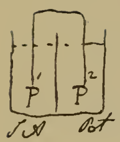
1752*. When the two Zincs were turned, i.e. taken out of the respective liquids and immediately immersed in the other liquids, the first effect at the Galvanometer was a current in the same direction as before, i.e. from Z^1 through the Galvanometer to Z^2 (which implies a current from Z^2 to the acid, then to the Alkali and then to Z^1); but in a moment this is reversed and a strong current sets from Z^2 through the Galvanometer to Z^1, then to the Alkali, next to the acid and again to Z^2.

1753. The final reversion of the current is consistent with the first observation that the Z in Alkali is more positive than that in acid. The momentary current produced directly after the change of the metals is due principally, I think, to the acid and alkali

* [1752]  changed to 

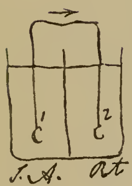
carried by the metals, and being that which, until it is displaced, acts upon them; and the Electrolytic conductor, instead of being S. A. against Z^2 and Potash against Z^1, is Potash against Z^2, then S. A. next, again Potash and finally S. A. against Z^1. So soon as these terminal but small portions of S. A. and Potash are neutralized by the surrounding Alkali and acid, then the current takes the direction consistent with the first result. The whole is therefore consistent.

1754. In order to free these and the other experiments of a similar kind from the supposed influence of the action going on between the acid and alkali at the paper diaphragm, Platina was next used, these pieces being not merely wires but large plates. The effect on the Galvanometer was so little as to be doubtful altogether. If existing, it was from the Alkali to the acid and then back from P^1 by the galvanometer to P^2. On washing and changing the platina plates the result was the same. On using fresh S. A. and solution of potassa still it was the same. Hence there is no sensible current of Electricity produced by the action of the acid and alkali on each other at the diaphragm. I have before given reasons why there could not be, for there are not Electrolytes to conduct on the power; i.e. the S. A. and Alkali are *ions* in the separate state and not combined, therefore no *electrolytic conduction* can be effected by them.



1755. In the former and future experiments therefore the current is the result of the action of the acid and alkali on the *immersed metals*.

1756. *Copper in S. A. and Potassa.* On using two pieces of copper wire cut from the same piece, the results were exactly as with the Zinc. That is, copper in Potash is more positive than copper in dilute S. Acid. The momentary effect on reversing the places of the copper was not so strong as with the zinc, but was very distinct.



1757. The relation of the copper to the acid and the alkali was much exalted by previous immersion of them in acid and alkali, i.e. copper from acid put into the alkali and copper from alkali put into the acid formed a stronger combination than if merely cleaned copper had been used.

1758. This effect may have relation to Marianini's effect, i.e. be due to the momentary current which has been formed in the

Full text of the letter from Whewell to Faraday.

Trin. Coll. Cambridge. May 6, 1834.

My dear Sir,

You will have received my letter of yesterday and perhaps will have formed your opinion of it. I still think *anode* and *cathode* the best terms beyond comparison for the two electrodes. The terms which you mention in your last shew that you are come to the conviction that the essential thing is to express a *difference* and nothing more. This conviction is nearly correct, but I think one may say that it is very desirable in this case to express an *opposition*, a contrariety, as well as a difference. The terms you suggest are objectionable in not doing this. They are also objectionable it appears to me, in putting forward too ostentatiously the arbitrary nature of the difference. To talk of Alphode and Betodé would give some persons the idea that you thought it absurd to pursue the philosophy of the difference of the two results, and at any rate would be thought affected by some. Voltode and Galvanode labour no less under the disadvantage of being not only entirely, but ostentatiously arbitrary, with two additional disadvantages; first that it will be very difficult for anybody to recollect which is which; and next that I think you are not quite secure that further investigations may not point out some historical incongruity in this reference to Volta and Galvani. I am more and more convinced that *anode* and *cathode* are the right words; and not least, from finding that both you and Dr. Nichols are ready to take any arbitrary opposition or difference. *Ana* and *Kata* which are *prepositions* of the most *familiar* use in composition, which indicate *opposite* relations in *space*, and which yet *cannot* be interpreted as involving a theory appear to me to unite all desirable properties.

I am afraid of urging the claims of *anion* and *cation* though I should certainly take them if it were my business—that which *goes to* the *anode* and that which goes to the cathode appearing to me to be exactly what you want to say. To talk of the two as *ions* would sound a little harsh at first: it would soon be got over. But if you are afraid of this I think that *stechion*, as the accepted Greek name for element, is a very good word to adopt, and then, *anastechion* and *catastechion* are the two contrary elements, which I am sure are much better words that you can get at by using dexio and scaio or any other terms not prepositions.

I expect to be in London Friday and Saturday, and if I am shall try to see you on one of those days and to learn what you finally select. Believe me

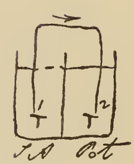
Yours most truly

W. WHEWELL.

reverse direction. Or it may be due to the clean and chemical condition of the metal wires. On looking at the two wires, the one from acid looked palest and most metallic; the one from alkali looked a little redder, as if a film of oxide were upon it. The difference was small, but still it might be enough to account for the effect, for the Alkali would have a perfectly clean surface to oxidize, and the coat of oxide in the acid might help to take the hydrogen from the water in the acid; for it is certain that, when the currents are formed, water is electrolyzed; and that oxygen is disposed of at the *anode* in the alkali and hydrogen at the *cathode* in the acid. This will be shewn well in the *Tin* experiment, and I have no doubt that Tin in solution of potassa could cause hydrogen to be evolved against Copper in dilute S. A.

Tin in S. A. and Potash.

1759. This metal acted as Zinc and Copper, but more interestingly. First, the Tin in Potash was always strongly Positive to the T. in Sul. acid. In the next place the tin required to be clean or the current would not be produced, and sometimes when it was apparently quite clean, still no current was occasioned. Then on a sudden the current would start into existence, and was always powerful and from Z^2 through the alkali and acid to Z^1.[1] When the current existed, a regular decomposition of the water occurred and hydrogen gas was evolved at T^1. This was not due to the action of the acid on T^1, but solely to the current; for when T^2 was taken out of the potash the evolution of hydrogen at T^1 ceased, and was again renewed by the re-immersion of T^2. Hence both chemical and galvanometrical proof of production and *direction* of the current and proof that the Tin in the Alkali determines it and is the Positive metal.



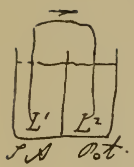
1760. On changing T^1 and T^2 for each other, the current through the cup was reversed for a moment, but quickly resumed its first direction as before. So that all right in that respect, and either piece of Tin could be made Positive to the other by immersion in the alkali.

1761. These experiments with Tin excellent. They are good proofs that metallic contact is not required. They afford also an excellent and wonderful case of the transference of the chemical powers,

[1] ? T^2 and T^1.

for in fact a piece of tin of any length can have its power of determining the combination of oxygen at one end made influential in determining hydrogen to the other end.

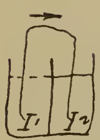
Lead in S. A. and Potash.



1762. Lead acted as the other metals, being positive in solution of potassa to other Lead in dilute S. A. The current was very fair at first, and hydrogen was evolved at L^1 in the acid, as in the case of tin; but not so abundantly. If the contact of L^1 with the S. A. be intermitting, a good deflection can be continued.

1763. On changing L^1 and L^2 in the divisions, then a little and only a little inversion of the current in the cup, but there was much strengthening of the principal current from the Alkali to the acid.

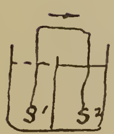
Iron in S. A. and Potash.



1764. Iron gave a current in the same direction with the other metals, but it was not strong. There was plenty of Hydrogen from the I^1 in the acid, but it was all chemical. It illustrates that the intensity of action there is nothing to the production of an electric current unless the power is electrolytically transferred onwards.

1765. It will be important however to consider in this case and in the case of Zinc, how it is that an affinity which can cause water to be decomposed that the metal may be oxidized, does not determine a current through the liquid toward the I^2. There is some important point hanging by this. It may however depend upon the iron presenting an infinity of little voltaic circles because of its heterogeneous nature. This by the bye is probably the true cause of the effect.

Silver in S. A. and Potash.

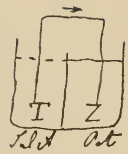


1766. There was scarcely a sensible action, but on very close examination the little current observed was, as in former cases, from the alkali to the acid in the vessel. When tried in fresh acid and alkali still the same.

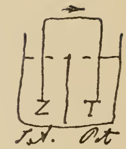
1767. Now tried a few cases of two metals and two fluids. Remember that in such cases no cause of action or *current* exists

at the contact of the two metals or of the two fluids, but only between the metals and fluids.

1768. *Tin and Zinc compared both ways in S. A. and Potassa.*

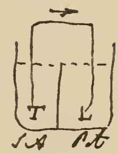


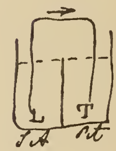
or



Whichever of the metals is in the Solution of Potassa is Positive to the other and determines a current from it through the liquids to the other. By former expts. (1697), if both Z. and Tin be in the same liquid, whether that be sol. of Potash or Amm. or dilute S.A., the Zinc is positive to the Tin. Hence power of alkali very evident.

1769. *Tin and lead.*

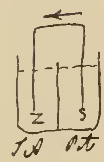


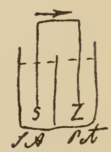


In both cases the metal in the alkali was positive to that in the acid, but the current was weak when the lead was in the alkali, and stronger far when the Tin was there. The lead in the Sul. Acid then actually evolved hydrogen whilst the tin touched it and was also in the alkali.

1770. When both the Tin and lead were in the Alkali, the Tin was much the most positive. When both were in the Sul. acid, there was very little difference. This agrees with former expt. (1703).

1771. *Zinc and Silver.*





Zinc was always positive to silver. But it was more positive when in the alkali than when in the acid. The current is always from the Zinc through the acid or alkali to the Silver when both metals are put into the same solution (1701).

1772. In these experiments the Sul. Acid and the potash gradually combined, forming a thick layer of crystals of Sulphate of potassa on that side of the paper only which was in contact with the solution of alkali. This might be a consequence of the acid being stronger than equivalent to the potash in an equal bulk of solution, but it is worth noticing as having always happened on this day. Note next time the relative value of equal volumes of the acid and alkaline solution, etc.

1773. The formation of such quantities of Sulphate of potassa without an enormous current of electricity, or even any current, shews that acids and alkalis in combining thus from the free state do not produce current.

1775[1]. May get the order of intensity of current formed by different solutions by means of this cup, etc.

[1] Par. 1774 is omitted in the MS.

1775½. Must make out how or why bodies in solution which, like Sul. Acid or Veg. acids or alkalies, give nothing to the oxidizing metal, still have different exciting powers.

1776. There may be a difference in the intensity due to the exciting cause and yet the quantity of electricity excited or transmitted be the same. Thus with Solution of Potassa and Sul. Acid, the first may excite electricity of a greater intensity than the acid.

1777. Has not the useful effect of the nascent state something in it analogous to the condition of a single circle?

1778. I sometimes think it may be possible to have a current of Electricity without a circuit, that is, to have an absorptive effect at each end of a series of apparatus. It would be a current between two vessels not forming a circle. Try Amalgam of Zinc and S.A. in one vessel and Copper in Sulphuret of potassa or in solution of sulphur in the other.

1779. Or the nitrate or chloride of Silver arrangement which makes the reverse current. Or silver in Hydrosulrt. ammonia.

1780. Considerations as to the intensities of the different forces which matter is governed by.

1781. The force of attraction of aggregation is as nothing compared with the force of attraction of chemical affinity. Hence chemical attraction so easily overcomes cohesion, and dilute sulphuric acid or even water, where the oxygen is already partly counteracted, will disintegrate and dissolve Iron.

1782. With reference to Mag. attraction, can a Magnet take iron or nickel out of a very fluid amalgam?

1783. Gravity is still far weaker. Must consider the relation of these three forces: Chem. attraction, Cohesion and Gravity.

Herschell's motions of Mercury between the Electrodes or Poles.

Combustion of diamond in oxygen a case.

1784. There is no case of the weakest chemical affinity being overcome by the strongest exertion of attraction of aggregation or even by Gravity. As to Hatchet's [? Hatchett's] cases of alloys, it depends upon other principles.

1785. Hence there is a most intense power active in all the cases of chemical attraction. Even in those of vegetation and animalization.

1786. Never *overcome* chemical by mechanical force, although we often *determine* its action. It is then probably incidentally.

1787. Chem. aff. as much surpasses aggregation as the latter does gravitation. Even Solution, which is so weak, can overcome aggregation.

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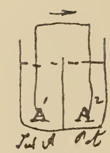
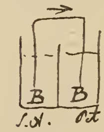
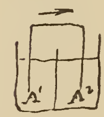
1788. Continued the experiments with one metal associated with dilute Sulphuric acid and solution of caustic potassa.



1789. *Antimony in Sul. Acid and Potash.* The effects were just as in former cases, the current from the Potash to the acid in the cell. On reversing the pieces, first the momentary current and then the permanent one, just as before.

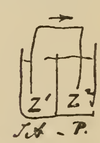


1790. *Bismuth in Sul. Acid and Potassa.* Just as with antimony and the other metals. Current strong at first.



1791. *Arsenic in Sul. Acid and Potassa.* The two pieces of Arsenic used were first heated in tubes, to drive off all the white arsenic and oxide and bring them to a perfectly metallic state, and then allowed to cool in the tubes. The effects were just as in the former cases. On changing A^1 and A^2 for each other, the first current lasted longer than in former cases and was stronger, but as the pieces of Arsenic were crystallized and shaken and as therefore more Sul. Acid and Potassa would by capillary attraction be held against and in them, that may be the cause.

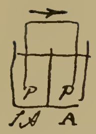
1792. *Palladium in Sul. Acid and Potash.* No effect. It gave no distinct current either way. There were some agitations of the needle, but these I refer to contact of the fingers, for which see notes at the end of this day's experiments.



1793. *Amalgamated Zinc in dil. S.A. and Potassa.* Strong current from the Alkali to the acid through the cup. The zinc in the acid did not evolve gas by itself, but the moment it was made to complete the circuit by contact with Z^2, then it evolved gas. Hence the chemical proofs of the current and its direction also present.

1794. Now repeated many of these experiments, using *Ammonia* in place of *Potassa* as the alkali, but retaining the same dilute Sulphuric acid. As solution of Ammonia is a very bad conductor, sulphate of ammonia was added to it. The ammonia solution was lowered about one half by water and the state of the liquids in

the two divisions of the cell continually watched to see that there was always plenty of free acid and alkali there.



1795. *Platina in Sul. Acid and Ammonia.* Large plates of platina were used. The current was distinctly from the Amm. to the S. A. in the cup, but there could not be any chemical action on the metal here.

1796. *Silver in Sul. Acid and Amm.* Much as platina. Current feeble but distinct, direction the same.

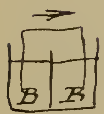
1797. *Palladium in Sul. Acid and Ammonia.* No action or current.

1798. *Copper in Sul. Acid and Ammonia.* Gave a strong current, the direction from the Ammonia to the acid in the cell, and therefore as with Potash.

1799*. *Lead in Sul. Acid and Ammonia.* Current in the same direction but not so strong as when potash used instead of the Ammonia.

1800. *Zinc in S.A. and Ammonia.* The current from the Ammonia to the S.A. in the cup and very strong. It was able to modify the power of Z^1 in the acid of decomposing water, for when contact was broken Z^1 evolved very little gas, but when the contact was made it evolved much. Hence chemical proof of the current as before.

1801. *Amalgamated Zinc in S.A. and Amm.* Just the same as in the last experiment, and as to the chemical proofs even more distinct, becauze Z^1 evolved no gas except when in contact with Z^2. Here therefore Potash and Ammonia the same, although perhaps the former the strongest.



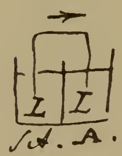
1802. *Bismuth in S.A. and Amm.* There was a current in the same direction as with Zinc, etc., but it was a weak current.

1803. *Antimony in S.A. and Ammonia.* As Bismuth: current weak but in the same direction.

1804. *Arsenic in S.A. and Ammonia.* As Bismuth: current weak but in the same direction, i.e. from alkali to acid in the cup.

1805. *Tin in S.A. and Ammonia.* Tin was remarkable for its action and in some respects much unlike that in S. A. and Potash. Yet a similarity exists. There was sometimes the appearance of a current, but it was uncertain; and on the whole the action was very weak and doubtful and the direction of the current as much one way as the other.

* [1799]



1806. *Iron in S. A. and Amm.* Again, Iron had remarkable effects. Sometimes the current was strongly one way, then strongly the other; but the general result was from the acid to the ammonia in the cup and the reverse therefore with that when S. A. and Potassa were used. These effects could not depend upon differences in quality, for both pieces used were the halves of one piece. They are probably connected with the superficial oxidation of the metal at the moment of contact, etc. etc., and seem to shew both that the forces are nearly ballanced, and also that there is a kind of inertia in the current when once produced.

1807. In all the experiments where the currents are feeble, it is needful to be cautious as to the source of the currents affecting the needle. The ends of the galvanometer wires were at first of copper, and when these were clean, touching them by the fingers of each hand deflected the needle. Even when the hands were washed still I could affect the instrument by holding one end and grasping the other at intervals only. Besides this, a motion of the wire across the magnetic curves of the earth can produce currents, etc. etc. I am inclined to think that some of the current with Platina, etc. etc. might be thus due to extra action.

1808. Must test the current from the Alkali to the acid, when Platina, Palladium, Gold and Silver are used, very accurately and carefully, to be sure that it really exists or not.

1809. The vibrations above by touching occurred even when the galvanometer terminations were of platina wire.

1810. Now made a few comparisons between *potash* and *ammonia.*

1811. *Zinc in Potash and Ammonia*, the latter containing Sul. Ammonia. The current was from the potash to the Ammonia through the cell and of moderate strength.

1812. *Lead in Potash and Ammonia.* Strong current, direction as with Zinc.

1813. *Tin in Potash and Ammonia.* Current weak, direction as with Zinc.

1814. *Copper in Pot. and Amm.* Current weak, direction as with Zinc.

1815. *Iron in Pot. and Amm.* Current feeble, direction as with Zinc.

1816. *Silver in Pot. and Amm.*
Platina in Pot. and Amm. } No sensible current.

1817. Hence Potash is stronger than Ammonia and Ammonia than dilute S. A. in rendering metals positive. But the action on iron very strange and must be repeated, for it seems to give an exception when ammonia is used.

1818. The little action, or entire absence, when such metals as Platina, Palladium, Gold, etc. etc. are used, seems to shew that the power of the acid and alkali is not to render the *metal* positive or negative or to induce any other state, but is to affect the oxide about being produced, or at least is related in its action to that oxide, or to the power the metal can exert in forming it.

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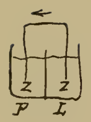


1819. *Single metals compared in different alkalies, etc. Zinc* in solution of *potassa* and clear *lime water.* Two thicknesses of paper were used to prevent too quick a mixture of fluids. The current was from the Potassa to the lime water in the vessel and moderate in strength.

1820. *Tin* in Potassa and Lime water. Current in same direction as with zinc and about same strength.

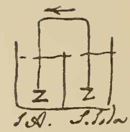
1821. *Lead* in *Potassa and lime water.* Current in same direction and of fair strength.

1822. *Iron* in *Potassa and lime water.* The current perhaps in the same direction, but the action was wavering and the current *very feeble.*

1823. Then in Place of lime water *cream of lime* was used, and this was stirred up from time to time. Zinc, Tin, Lead and Iron were tried again and gave currents in the same direction as before and of about the same strength. Iron was very uncertain and doubtful.

1824. Now *Ammonia* was used in place of Potassa against *cream of lime.* With Zinc the current was from the lime to the ammonia in the cup, but very doubtful and wavering. With Tin and lead it was from the *ammonia* to the *lime*, but very feeble. If any consequences were to be drawn of importance, the expts. should be repeated with impervious paper or paper nearly so, for a little potash or ammonia could pass the paper now uzed.

1825. Now tried dilute S.A. s.g. 1·300 against a strong solution of sulphate of soda, using two papers and taking care to keep the saline solution neutral by adding a little soda to it now and then.



1826. *Zinc* in S.A. and sul. soda solution. The current was from the Sul. Acid to the salt, but not at all strong.

1827. *Tin.* The current in the same direction, but very feeble.

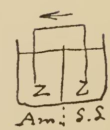
1828. *Silver.* No sensible current.

1829. *Copper.* Very uncertain current, but generally from the solution of Sul. Soda to the acid; now and then strong, but hestitating, as if some unsteady equilibrium were overcome, first one way then another.

1830. *Iron.* Very fairly and constantly from the solution of Sul. Soda to the acid. This very curious as well as the copper. Must repeat if require the experiment to quote.

1831. *Lead.* Scarcely any current at all.

1832. Now tried sol. *Ammonia* against sol. *Sul. soda*, but of course during the experiments a little mixture, etc. would take place.



1833. *Zinc* in *Amm.* and sol. *Sul. Soda.* Current very feeble: was from the Amm. to the sol. Sul. Soda.

1834. *Tin* in Do. Current very feeble: not sure, but apparently in the same direction.

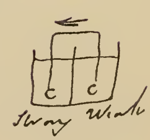
1835. *Silver* in Do. Current feeble but from Am. to S.S.

1836. *Copper* in Do. As Silver.

1837. *Iron* in Do. Current and effect insensible.

1838. *Lead* in Do. Current and direction insensible.

1839. Now compared *Strong and weak Sul. acid.* At first put in the S.A. s.g. 1·336 on the left side and only water on the right, letting that water get a little acid by the mixture through the two papers. Then after one set of observations, strengthened the weaker acid a little so that it might taste sharp and sour, but still not making it more than $\frac{1}{20}$ the strength of the strong solution. The results were the same in both cases and as follows.

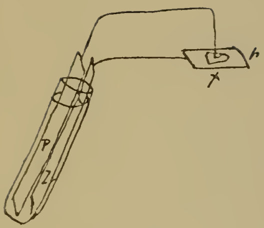


1840. *Platina* in strong and weak S.A. There was no sensible current. *Silver* produced no sensible current.

1841. *Copper.* A distinct current, though feeble; it was from the strong to the weak acid.

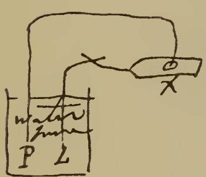
1842. *Tin.* As copper in direction of the current, but the current itself stronger.
1843. *Lead.* Current fair, and from strong to the weak acid.
1844. *Zinc.* Good current and from the strong to the weak acid.
1845. *Iron.* Current was very uncertain and feeble, wavering much.

28 MAY 1834.

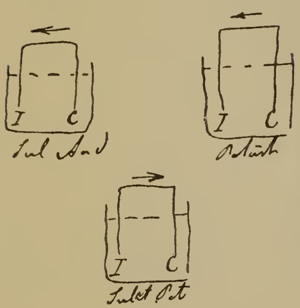


1846. Expt. to try if heat would, by exalting chemical affinity, increase initial intensity of a current. A zinc amalgamated and platina plate separated by paper were put into hot dilute S.A. s.g. 1·25. The zinc was connected with a platina plate *p* on which was put a drop of Mur. acid blued by sul. Indigo, and a platina wire from P touched it at *x*. There was a gradual action and the indigo lost colour, so that it is probable an effect was produced. The expt. is worth pursuing therefore, but I can not spare time for it here. It should be tried on sol. sul. soda or fuzed chloride or iodide of lead.

1847. An amalgam of potassium put into *water* gives a strong current with a plate of platina, the current being from the amalgam through the liquor to the platina. Here could be no action of an acid or alkali on the oxide formed: the electricity all from the oxidation. See Davy on same effect, though not accompanied by the same reasoning.



1848. Again, a plate of clean lead and a plate of platina put into pure distilled water, with only a piece of filtering paper between, gave a current which readily decomposed sol. iodide of potassium at between the platina wire and plate there. Here no action of the oxide formed any acid or alkali. The current was from the lead to the platina in the solution.



1849. *Iron and copper* in *S. A.*, in *Potash* and in *Sulrt. Potash.* When Iron and copper are connected in dilute Sulphuric acid, the current is feeble but from the Iron through the cup to the copper. When the same metals are in solution of potash the current is also feeble and in the same direction as before. When they are in a solution of sulphuret of potassa, the current is reversed, being from the C. to the Iron through the acid and pretty strong. In this case a sulphuret of copper is quickly and beautifully formed,

especially whilst the copper is in contact with the iron—but no sulphuret of iron is produced.

1850. When the two pieces of iron and copper which have been in the sulphuret of potassa are taken out, washed, wiped and, being communicated with the galvanometer, are put into either dilute Sul. Acid or sol. of potash, a very powerful current from the iron to the copper through the fluid is produced, the two metals having been highly exalted with regard to each other by previous use in the sulphuret. It appears to be the copper which is thus exalted, but the effect does not continue until all the sulphuret is gone; for it lasts only for a short time and then the iron and copper are very feeble towards each other in the acid or potash, but are exalted as to their relations in the sulphuret. Davy, Phil. Trans. 1826, p. 395.

1851. *Spark from single pair of plates on contact.* Used a new Electromotor consisting of a double cylinder of copper and one of Zinc, the latter amalgamated. The acid dilute Sulphuric, S. G. about 1·25. A copper wire was fastened to the copper cylinders and brought against the clean filed edge of the zinc cylinder. There was no action of the plates before contact. There was a spark on making contact quite as bright as the spark on breaking. The points of contact must be clean and dry. On amalgamating the points of contact the spark was much finer and larger for the reasons before given.

29 MAY 1834.

1852. A weak battery used and silver poles in a solution of sulphuret of potassa. Plenty of sulphur was set free at the *anode* and much combined with the silver. I do not believe that this sulphur is a secondary result, but a direct result, because it passes in the same direction as when copper and iron are put into a solution of sulphuret. Sulphur is in both these expts. and in that on sulphuret of silver an *anion.*

1853. Made a solution of seleniuret of potassa. It when acted on by battery gave selenium at the *anode.*

1854. When copper and Iron were put into this solution of seleniuret potassa, the current was from the iron through the solution to the copper, and therefore the reverse of that in sol.

sulphuret, but then the copper was very little acted upon—nothing like what it was in sulphuret solution.

1855. Silver is more acted upon I think in the Selenium solution than copper, and when Silver and copper are put into the solution the current is from the former through the electrolyte to the latter. Hence proof that selenium is an *anion.*

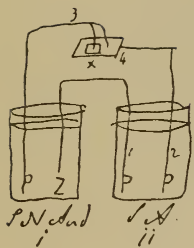
1856. Silver and copper in Sulphuret of potassa give a reverse current, but still the sulphur is an *anion*, for now it goes to the copper.

1857. *Great points in Electricity which require to be decided.*

Is not the existence of compound *ions* assumed rather than proved? Has an acid or a base yet been determined to the electrodes except in a solution, and would they go in equivalent proportions in ordny. salt? In fact is it not the simple bodies only which truly and freely traverse? This not yet definitely decided.

1858. If there are; still, may we not by Electrical relations of the simple *ions* distinguish between real elements and such as we may think to be such because we have not decomposed them? That is, will not electricity prove to be the test between bodies really simple and those which are compound? If so, probably our present elements are true and ultimate elements.

5 JUNE 1834.



1859. To ascertain if current by S.N.Acid would be intense enough to decompose water by the apparatus described 1691, 1695, 1728, etc. In i was put dil. S.A. with a few drops of N. Acid; in ii only dilute S.A. s.g. about 1·3 or 1·25. There was a good current indicated by the effect on the iodide potassium at *x*. In less than five minutes gas appeared both on P^1 and P^2 in the second vessel. Supposing this might be due to some action of the plates as nuclei, the connexion between 3 and 4 was broken and 1 and 2 cleared from bubbles and all left in this state for 15 minutes, during which no bubbles appeared on either. The current was then restored by contact of 3 and 4, and in a minute bubbles were evident on 2 and in five minutes it was covered. On breaking contact at 3 and 4, clearing 2 and restoring it to the

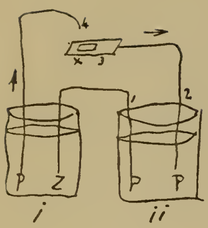
solution for 15 minutes, no bubbles; but on renewing the current in 5 minutes plenty of bubbles.

1860. In this experiment there was more appearance of bubbles at the anode in ii than at the cathode, i.e. more appearance of oxygen at 2 than of hydrogen at 1; and again at 2 there were more bubbles on that side of it which was towards the light and farthest from 1 than on the other side. Thinking this might be due to an aiding action of light, 2 was taken out and twisted half way round so that the sides should change their relative situations, and the current again restored. The gas was now evolved more on the side in the dark than on the other side, so that that side had an advantage in structure or some other point, but the difference was by no means so great as before. And it is well worth ascertaining whether light does not aid a feeble current or affect its power of decomposing in some way.

1861. From the expt. altogether it is well shewn that the current by S.N.A. can decompose water, whilst that from S.A. alone cannot. Hence good proof of the necessary intensity required.

1862. Arranged another apparatus as above, but put a strong solution of Potassa into i instead of Sul. acid. The zinc used in this and the former was amalgamated zinc. There was a good current at *x*, but the metallic communication was completed and retained for 96 hours, or four days (without any interruption except for a moment at a time to prove the continuance of the current), and yet there was no appearance of decomposition in ii. Hence the intensity of the current produced by potassa is not enough to electrolyze water. The apparatus was finally left for 14 days, and though a current had passed the whole time no decomposition of water had resulted.

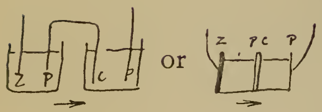
1863. Made an experiment to ascertain whether, with currents below the electrolytic intensity for water, pure water did not conduct as well as acidulated. An apparatus arranged exactly like that just described was used, but i contained dilute Sul. Acid and ii pure water. By observing the decomposition at *x*, there was no appearance of obstruction by the water more than by acidulated water, but rather indeed the reverse. The advantage of pure water appeared to depend upon the platina plates in it not acquiring that peculiar state which makes them tend to produce a counter



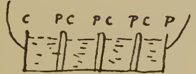
current (Marianini's state) to the same degree that they do in the dilute S.A. It is easy to examine this state in the apparatus, thus: let 4 touch the metal plate 3 (platina); a current runs round which makes P[1] and P[2] acquire the peculiar state. If 4 be then lifted off 3 and put on to the paper with sol. iodide potassium at *x*, it will be found to exert but little decomposing power, if any; but if it then be lifted up, and whilst up 1 and 2 be connected a moment by a platina wire, their counter power is discharged, and on putting 4 on to *x* quick powerful decomposition occurs. By making 4 touch 3 again for a few moments, the passing current restores the peculiar states of 1 and 2 and the effects can be repeated.

1864. Now these effects are much less feeble when water is in ii than when acid is there, and hence the reason why pure water is a better conductor apparently than acidulated water. All the phenomena are better observed with plates and an apparatus in which the current has been circulating for a while. The plates after that more easily take up the peculiar state. After 4 days the current was passing through the water but no signs of decomposition in it had taken place. Nor after 14 days.

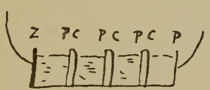
1865. Experimented with interposed plates of copper in order to explain and reconcile the results at 1541, 1595, 1596.



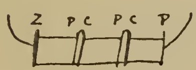
This arrangement gave a good current.



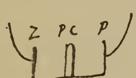
Four pair of copper and Platina thus arranged and always uzing the dilute S.A. gave no sensible current.



Putting zinc for the extreme copper plate so as to make the apparatus like the first, but with three interposed P. and C. instead of one, gave a very feeble current, scarcely sensible.

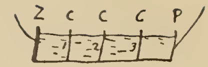


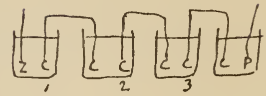
Taking out one of the pairs of platina and copper, a current passed much stronger than the last, but still feeble.



Taking out another pair so as to return to the first state, then a moderately good current passed.

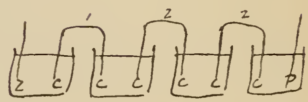
Now instead of three platina and copper pairs, in the order in which they would tend to assist the current forward as to direction at least, used only three interposed coppers, having the form as before, and expected this would give feebler current, as the copper surfaces 1, 2, 3 were those places where hydrogen would tend to appear. But the current was very good, being scarcely at all diminished.





1866. I found however that if the apparatus was left a little while communicated, the current soon ceased entirely, and then the coppers were as platinas or as copper and platinas.

1867. This effect depended apparently on a peculiar state assumed where the current passed into and out of the surfaces (Marianini), giving them the power of producing a counter current; and when the apparatus, being connected with the galvanometer, was all quiescent, if the copper 1 was taken out and turned round, a current was produced. If two or especially if the three coppers were turned round, the current was very powerful for a while, and then sank to nothing; at which time, if the coppers were returned, the current was again renewed in the same direction.



1868. Hence, and with the oxidation, etc. of Zinc in its ordinary state, etc., a reason for the anomalous effects in 1541, 1595, 1596.

1869. There seems to me little doubt but that this is still a part of Marianini's state, but it seems to me to connect it with conditions of the surfaces caused by the precipitation or evolution of matter on them. Remember the extremely minute portion which would suffice to produce this quantity of Electricity.

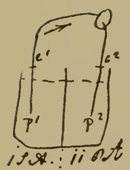
1870. Experimented on the gas voltaically evolved by amalgamated Zinc connected with a platina plate and acted on by Potassa solution. The form of the experiment was just like that before described with Acid, but the time required was much longer, lasting from 3 hours. All the Hydrogen was evolved on the platina and therefore electrolytically, but as it rose it ascended against the zinc, which hung as it were over the platina. The plate of zinc left in the same solution of alkali by itself weighed at first 164·55 grains, and at the end of the three hours 164·52 grains; it had lost therefore essentially nothing. The other plate, which weighed at first 175·5 grains, was reduced to 167·95 grains, having had 7·55 grains oxidized and dissolved by the solution of alkali.

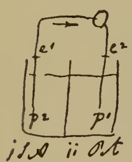
The quantity of gas collected was 10·8 c: i: at 64° F., and Bar. 29·8 with a water pressure expanding the gas of 5·5 inches = 0·4 of mercury and therefore = Bar. 29·4 inches.

1871. The gas corrected for temperature, pressure and moisture becomes 10·274145 c: i: of hydrogen, which, increased one half for the oxygen, gives 15·4112, etc. of O. and H. from water decomposed. This brings out the number for zinc a little above 34, which is so near as to shew the truth of the principles, etc. etc. advanced.

1872. Must observe that the power of acids, salts, chlorides, etc. to increase conducting power of liquids affects them only as electrolytes.

6 JUNE 1834.





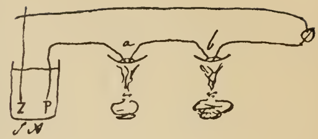
1873. Experimented to ascertain whether platina electrodes in acid and alkali really produced any current. Davy says they do: P. Tr., 1826, p. 404. I used the glass halved vessel and cleaned the electrodes well first with emery and a cork. On immersion there appeared to be a current from the alkali to the acid. This current quickly fell to nothing, but if then P^1 and P^2 were taken out and changed side for side and then again plunged in as in the second figure, there was a still stronger current than before, but in the same direction, which quickly fell to nothing and could then again be restored by changing sides. This effect is probably partly due to electricity from the combining acid and alkali, partly from that which the plates can produce in consequence of having had a reverse current through them, and the whole result is I suspect complicated; but there did seem to be an excess of action, as if a current set in the main from the acid to the alkali. These currents have nothing to do with the essential electricity of the Voltaic pile.

9 JUNE 1834.

1874. Have been experimenting on the intensity necessary for decomposition and on conductibility below that degree.

Amalgamated Zinc and a platina plate immersed in dil. S.A. without N.A. gave origin to the electric current. The current was passed through a galvanometer and sometimes through one and

at other times through two places of decomposition *a* and *b*. At first chloride of lead was placed at *a* and chloride of silver at *b*, both fuzed on glass by lamps beneath. A very feeble current passed through both, I think without decomposition in the chloride of lead, but I cannot tell as to the chloride of silver; it was so dark in colour and also so little electricity passed.



1875. Now the fuzed chloride of lead was left at *a*, but at *b* was placed a plate and a wire (platina) with paper moistened in sol. iodide Potassium there in the usual way. There was a moderate decomposition of the iodide, shewing that the current passed the chloride at *a* and decomposed at *b*; yet when metallic contact was made at *b* the chloride of lead did not appear to be decomposed. It seemed to conduct without decomposition.

1876. Then fuzed chloride of silver was retained at *b* and the test apparatus introduced at *a*. Now there was very quick action at *a* on the iodide, as if a strong current passed, far stronger than with chloride of lead intervening. I thought also there were appearances as if the chloride of silver was electrolyzed by this current and did not merely conduct as the lead chloride did.

1877. Then arranged so as to have but one place of decomposition, as for instance *b*, with the fuzed chloride of silver there, and put the two ends of the wires as near together as they could be without touching. Leaving all thus, there was at first but the smallest indication of a current at the Galvanometer, but in a minute or two the needle would swing round and indicate a strong current. This I attribute to the reduction of the chloride of silver by the current and the completion of the circuit metallically by the reduced silver. The process is a very good one to assist in giving assurance of reduction by a current of given intensity, i.e. produced by amalgamated zinc on dil. sul. acid.

1878. Made the same experiment with chloride of lead but could not get the same evidence of reduction. But then lead fusible at that heat, and therefore would not extend across the Electrolyte as silver does. Still, I think the chloride of lead is not reduced by this current, but conducts without.

1879. Now arranged matters, putting at *a* the test apparatus and at *b* a little pure water in a watch glass to represent the fuzed bodies, and having only the small platina wires to complete

communication instead of plates. In 5 minutes and even in 2 minutes decomposition of the iodide shewn; hence water conducts even here, though not decomposed, but time longer than with the other apparatus (1863).

1880. Arranged the test apparatus at *a* and fuzed nitre at *b*, taking care that the heat merely fuzed the salt and did not decompose it. The nitre conducts pretty well and decomposition of the iodide follows. Sometimes it fails. I do not know why; probably from want of contact in some of the wire: it is very important that with these low currents contact should be quite *sure*. Often when the wires seem to touch there is none because of a film of oxide or dirt.

1881. The nitre conducts better than water, but I could not perceive any proofs of its own decomposition. No discoloration of the platina nor any gas evolved.

10 JULY 1834.

1882. Experimented to ascertain the bad effect of a weak or exhausted trough amongst several strong ones. Four troughs were newly charged and arranged with a fifth containing an old and exhausted charge so as to form one battery. A volta electrometer was arranged, the plates put down and the current of the whole five troughs sent through the instrument for one minute. One of the wires was then moved so that the current of the four good troughs only should pass. In the first case only 1·1 cubic inches of oxygen and hydrogen was obtained; in the second 8·4 cubic inches. So that the introduction of the one weak trough had reduced the power of the associated troughs nearly $\frac{7}{8}$ of the whole amount under the circumstances.

1883. Another expt. was made with the whole and with different parts of the battery for exactly equal times, namely half a minute in the order set down.

The whole 50 pair of plates evolved only	0·9	of cubic inch.
The 40 well charged	4·6	"
The whole 50	1·0	"
The 10 weakly charged pair	0·4	"
The whole 50	1·15	"
The 40 well charged	3·0	"

The general result is here very evident; the gradual fall in force of the 40 is due to their action on the plates and consequent deterioration. The gradual increase in power of the whole is due to the increased temperature in the weakly charged trough by the continually passing current and its consequent increase of force.

1884. In the Voltaic pile, when circulation is complete, the Electricity is not formed or evolved, only arranged. Must consider well that in this state it has no tension.

1885. Electricity of friction: its analogies, 1646, 1647.

1886. Though the quantity of electricity which passes when equivalents of two different bodies are decomposed is the same, the true or free force required may be very different and relates to the *intensity*.

1887. Must explain the irregularity of current obtained with nitrate or chloride of silver referred to at end of 476 par. of printed series.

1888. Must consider also 526 printed series, and explain clearly how the acid travelled.

1889. Must consider solid decompositions vii, Sulphuret of silver 871, and ascertain how they are related to definite action of electricity.

1890. Are any solids (which can decompose) structurized by the effect of the passing current; 319, 494, 501-504?

Ampère, Ann. de Chimie, li, p. 78.

1891. As to requisite intensity of current for decomposition, put the following from old notes as perhaps useful: Hachette, Ann. de Chimie, li, p. 74; Nobili, his third paper, conclusions, p. 34. When current passed, decomposition always takes place, says Nobili, p. 9 of translation of his first paper or p. 34 of his third paper. Matteucci says *not*; New Quar. Journal, i, p. 371 [? p. 612]. Volta says not, Phil. Trans., 1829, p. 16. Hachette?. Biot on necessary intensity for decomp., Ency. Britt., Vol. 4, p. 440, column 1, article Galvanism. Was there in that case perfect insulation or not, i.e. was there a current not enough to decompose? Same place and page, bottom of column 1, is the difference of gases evolved in solutions of nitre and Glauber not due to other action? Salts compensating—this considered in relation to equivalency of chem. action.

1892. Try nonconducting power of a diamond when very hot.

1893. General relation of conduction to compound nature of bodies: is there any, or only partial and accidental?

1894. As to the great question: is it possible the *sum* of Electro chemical decomposition can vary? Decide this by use of sulphate of Soda or salts of baryta or some body not subject to secondary action tried against Sul. Acid. See 280, 521-523, 555, 559, 568, 600, 628-630, 632, 633, 681, 851, 852, 976, 978-982, 1247-1249.
1895. Try Zinc Pos. Electrode in Neutral sul. magnesia.
1896. As to general action of all the elements or matters present. It would seem rather that only one gives way and transmits the electricity unless the others rise to high proportion, and they can so rise *against* the electrodes, i.e. in the part of the fluid in contact with them. See 506, etc. Also P. Trans., 1807, pp. 18, 19.
1897. Agreement of my views of electro chem. decomposition as to internal action with all the newer facts; 802, 804, 869, 1188.
1898. Independant nature of Electricity: is not consumed or exhausted in any of its peculiar action; only transferred onwards.
1899. Probable effect in nature, trees, etc., 105, 407, 414. P. Tr., 1807, p. 53, Note. Water probably the only body electrolized here—nascent state of its elements. Query cause of buds pointing upwards, roots downwards.
1900. Nervous action.
1901. Selected poles—with reference to principles of transfer, as for iodides, sulphurets, etc., 390, 401. Perhaps decompose bodies not yielding to platina poles by others of Zinc, Tin, etc. Try iodides, sulphurets, artificial camphor, etc.; but not likely such as the latter give way.
1902. Plumbago: is it always pure carbon? Why found against iron? True nature of carbon? Plumbago from cast iron by acids.
1903. Propose to restrict the term of Voltaic combination to cases where an electrolyte is present and acting as such. May, in the various modes of action of chemical powers, be others between common action and voltaic; but if so, use other terms for these, as they involve other principles of action.

5TH AUG. 1834.

1904. Certain *laws* of action (according to those named) of Electric, Electro-magnetic and Electrolytic action.
1905. All parts of an equal and homogeneous conducting wire

carrying a voltaic current exert equal actions. *Savary*, Ann. de Chimie, xxxiv, 49. Also Becquerel.

1906. Two wires of the same substance transmit with equal intensity the same voltaic current when the ratio of their lengths to their transverse sections are the same. *Davy*, *Becquerel*, Ann. de Chimie, xxxiv, 21.

1907. Relation of Moving and magnetic effect to the quantity of electricity passed. *Herschel*, Phil. Trans., 1824, p. 168.

1908. The conducting power of metal for common electricity is not influenced by the *form* of the metal, i.e. whether cylindrical or flattened or separated into form smaller: still the same. *Harris*, Phil. Trans., 1827, p. 23.

1909. The Voltaic (Magnetic?) effect of two batteries of the *same length* and *same size of plates* is *directly proportional* to the number of plates. Each pair of plates produces an equal effect in whatever part of the battery it is placed. *Ritchie*, Phil. Trans., 1832, p. 289.

1910. The magnetic effect of the current of a pair of plates is inversely as the square roots of their distance in a trough of equal size with the plate. *Ritchie*, Phil. Trans., 1832, p. 289.

1911. Two batteries with plates of equal size and at equal distance have their energies (magnetic and chemical) very nearly proportional to the square roots of the numbers of (pairs of?) plates. *Ritchie*, Phil. Trans., 1832, p. 289.

1912. Several other laws as to relation of Magnetic deflection to the electricity passed. Quar. Jour., N. S., i, p. 29.

1913. Some conclusions, general, of my own to consider, etc.

That very possible for the same quantity of Electricity to be disturbed or transmitted yet not exert the same degree of force.

1914. Remarkable that though the quantity of electricity concerned between the electro-chem. equivalents of oxygen and sodium and chlorine and sodium must be the same, the forces of attraction are not the same; or, as far as we know, the intensities of the currents they can determine.

1915. But considering the force of the electricity upon a magnet whilst passing through a copper wire as a true and constant measurer; it would appear that the electricity of the equivalents are the true forces, and that the differences in the power of the particles are due to the mode of its association with them.

1916. Quantity in Electricity appears to be analagous to the pitch in sound or the colour in light, and intensity in electricity to the loudness of the sound or the vividness of the light, i.e. to associate with increased energy of vibration but not with more numerous vibrations.

1917. The electricities appear to be the forces of attraction by which two particles combine.

1918. In reality no compound Electrolytes have as yet been *really* and *equivalently* decomposed.

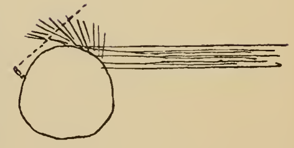
1919. I must give soon my present view of the theory of Electro-chemical decomposition as a whole.

19 AUG. 1834.

1920. *Zodiacal light.* Whilst at Walmer last month and the beging. of this, I witnessed the Zodiacal light twice, and have some suspicion of its cause. The ray was like an acute spindle, larger below than above and not at all like a ray depending upon the shadow of the clouds. It was *vertical.* The sky was pretty clear—indeed very clear, except that three or four thin streaks of very light cloud ran across the ray, and these were most distinctly more luminous where the ray crossed them than the rest of the ray, i.e. those parts of the ray which intersected the clouds were more luminous than the other clear parts. Hence I conclude that the ray depends upon light coming to the eye from particles in the atmosphere of the earth, and that in this case, the crossing clouds presenting more particles, reflected more light than the clear parts.

1921. If this be so, then the Zodiacal light would not be the Visible projection of a disc or ray of light surrounding the sun in one direction, for such a ray would be visible to an eye placed out of the earth's atmosphere, if the sun's body could be shaded from it; but must depend essentially on our atmosphere. In that case it is a terrestrial phenomenon in part and not purely a solar phenomenon.

1922. May it not be due to light reflected from the surface of the earth and the waters, etc. thereon, which being thrown off at an angle of incidence equal, strikes upon and illuminates the haze and so produces the luminous image: and would not the reflection be such as to make the light more distinct in the



(vertical) plane containing the observer, the sun and the center of the earth, and so produce the narrowing of the image and the appearance of the ray as a spindle and not a diffuse twilight or illumination?

Can experiment on the latter point.

1923. Mentioned my view to Mr Christie on the Evening of the 13 Aug., 1834, when he told me of his observation of a vertical ray, expanding upwards, which appeared from *behind clouds* and which he referred to some effect of repeated reflection from the *clouds*. He did not consider what he had seen as the Zodiacal light, nor did he think it was due to reflection from the earth's surface, but the clouds' surface, etc.

21 AUG. 1834.

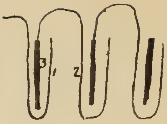
1924. Have been considering of the new construction of the battery and have given Newman orders to prepare the trough and plates. It is quite clear to me that when the copper surrounds the zinc, there is no use in or occasion to insulate one copper from its neighbour, as the copper 1 from the copper 2: the electrolyte which is between is a sufficient insulation, for several reasons. In the first place, the coppers 2 and 1 have of themselves equal chemical relation to the liquid between, and will not therefore induce any current or electrical action. In the next place the relation of 2 as an electric exciter, etc. is to the Zinc within it and not to the copper without it. In the third place, if there were a tendency of an electric current backwards from the copper 1 to the copper 2, it could not pass because the power required to decompose an electrolyte, i.e. the fluid between 1 and 2, is more than a single pair of plates can produce; and only a single pair can be active in urging the current backward between two consecutive coppers.

1925. References on this subject of the battery.

Pepys apparatus for raising and lowering the plates. Quar. Jour., i, p. 193.

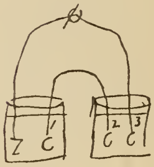
Hare's revolving battery. Quar. Jour., xvii, 378; or Phil. Mag., lxiii, 241.

Hare's Calorimotor. Ann. Phil., N. S., i, 329; or Gray's Operative Chemist, p. 222, etc.



1926. The question is whether 3 can discharge to 2 through 1. Can try that by an expt.

23 AUG. 1834.



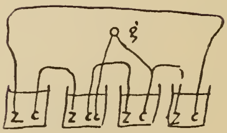
1927. Arranged two cups with Zinc and copper as in the figure, with dil. S. A. in the cups. There was a current on first immersion, but it soon fell. On taking out the plates and re-immersing them, there was again a current, but it fell again. When C2 and C3 touched, the current was extremely more powerful. Hence there would be back discharge in a battery of Zinc and copper charged with dil. S. A. at the moment when the charge first put in.

1928. But supposing the ends of the battery connected so as to open the right door for the current, it would probably be much less: perhaps even nothing important.

1929. When Platina was used instead of C1 and C2, then scarcely any current was obtained, so that a battery of Zinc and Platina would not have back discharge.

1930. When a little N. A. was added to the S. A. in the vessels, then there was a constant current in the apparatus, but it was small compared to what passed when C2 and C3 touched. Still it shews there would be a continual loss, though how little that might be when the poles of the battery are connected is not so clear.

1931. It is still well worth while making the expt. and ascertaining how well such a battery will act. With the power of pouring the acid on and off, it may much surpass common batteries.



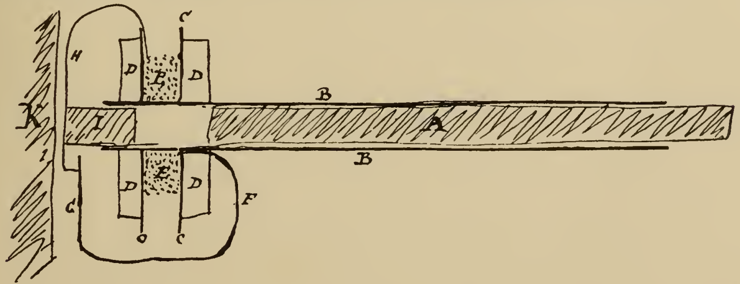
1932. Must try this expt.: for that is the true state of the case. When the electrodes are unconnected, there will be a current through the galvanometer *g*; but what will there be when they are connected?

25 AUG. 1834.

1933. To-day procured the Electric spark from a Magnet directly, i.e. used no soft iron lifter or other intermediate magnet. The arrangement is here shewn in section*.

A, a cylinder magnet 10 inches long, $\frac{8}{10}$ in diameter; it would not lift more than 2 oz. at either pole. B a pastboard cylinder in which A could move freely. C, C, two collars of pastboard backed

* [1933]



by the two bungs D, D, which were fastened by sealing wax to the cylinder B. E, about feet of copper wire covered with white silk and coiled between the collars, C, C. F, one end of the coil soldered to a copper plate G. H, the other end of the coil, bent so as to rest upon the plate G and bear against it with a little spring. The wire and plate where they touched were amalgamated. I, a piece of wood which, when urged forward by the magnet A, would separate the wire H from G at the most favourable moment of time. K, a wall to stop the magnet.

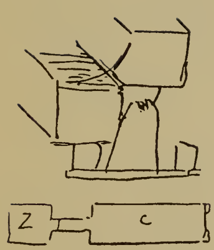
1934. When A, being partly out as in the figure, was driven quickly up the cylinder, it opened H from G before its motion was concluded, and the spark was seen there. The Electricity here is much more directly from the magnet than in the usual way of procuring a spark.

8 SEPTR. 1834.

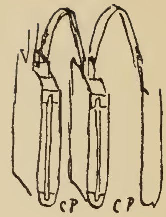
1935. Endeavoured to examine crystal of lead by polarized light. Was able easily to arrange a watch glass between my tourmalines and so get a ray through, but did not procure a good crystal. All the crystals when examined closely were dendritical and not plates, and too opaque in the solid parts. I used only ten pairs of plates: think I shall do better with 40 or 50 pair and a stronger solution of the acetate. Crystals formed slowly appear to be more solid than those formed quickly.

Should also put on a lead tree: perhaps get some good crystals there.

13 SEPTR. 1834.



1936. Have experimented on the new construction of trough. Newman has made me a wooden trough 18 inches long, $3\frac{1}{2}$ inches wide and $3\frac{5}{8}$ inches deep, on Hare's construction, so as to revolve 90° and put the acid on or off the plates. He has also prepared me 40 pr. of zinc and copper plates of right width and size, so that I could bend them over a mould and make them agree together. Three plates of plate glass were used, one at the bottom and one at each side of the half trough which was to receive the plates. The plates were bent, pieces of cork adapted to the four corners of the zinc plates and then the whole packed in the trough



thus; making in fact Wollaston's arrangement, but then there was only a double thickness of cartridge paper between the different coppers, instead of an impervious division; and the plates being all pressed up together, the 40 pairs only occupied about 14 inches or 14·3 inches in length of the trough. The Glass plates were then wedged up by pieces of wood on one side. The space at the end was filled up by corks to keep the plates tight. The end plates were connected with the copper pivots on which the trough turned, and these working on copper bearings, gave opportunity of using fixed poles.

1937. A charge of ordinary acid was then put in the vacant side and the trough used for various expts. of decomposition, heating, etc. etc. The connection of the apparatus with the poles was made first—then the acid was poured on to the plates—the effect obtained—the acid passed back again and the whole left in a quiescent state. The apparatus decomposed water well and rapidly—it gave an excellent spark—it heated a good length of wire—it burnt metals very well—it gave a fair shock. It did I think the work of 40 pair of three inch square plates well, and was used in this way for continual experiments for 45 minutes. At the end of that time the power was weak, and on tasting the liquor it was scarcely acid sensibly. It had no green colour and had dissolved little or no copper. It was removed, water poured over the plates—that removed, and the whole left to itself.

1938. The quantity of dilute acid used was not more than 3 Pints—not enough for a single trough of the usual construction of ten pair of plates—but acting on 40 pairs, it was no wonder that it was so soon neutralized. Thus all the effect of the acid was obtained, and a very small quantity does.

1939. It is quite clear that the construction is good in principle and that the coppers need not be separated more than not to touch.

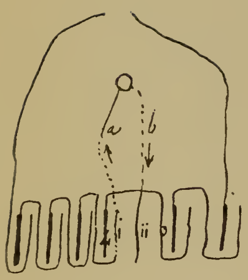
1940. The wood was oiled—the electrodes had better descend to the edge and front of the stand. Oiled thin millboard, pastboard or even paper (cartridge) better perhaps between the coppers than unoiled.

1941. Must have one made with plates in a frame to drop into a box of acid. Must have a plug at the corner of trough to withdraw the acid by.

1942. Advantages. Compact powerful form and effective instrument. No annoyance of gas evolved. Obtain the effect of first immersion. Have a uniform charge always. No copper dissolved and precipitated on the zinc. No waste of force between the experiments. Economy of zinc and consequent lightness of a 50 pair battery. Economy of acid. Ready manipulation.

1943. If want more [illegible] effect, then have plates in a frame and drop them into the acid.

15 SEPTR. 1834.



1944. Worked with the new trough of 40 pr. 3 inch plates, paper only being between the contiguous coppers. The object was to ascertain what took place between the coppers, and how the electricity passed, if any passed there. For this purpose a single copper plate was put between two contiguous coppers, but with the double thickness of cartridge paper between it and the copper on both sides, thus. A wire proceeded from that plate and was connected with a galvanometer, and another wire from the galvanometer was made to touch any desired part of the battery.

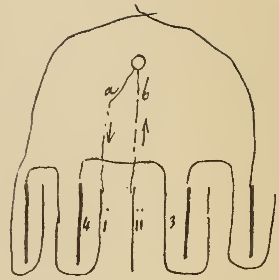
1945. When the *electrophores* were not connected and there was consequently no current through the battery, and the wire *a* was made to touch the copper i, there was a pretty strong current from i, through the galvanometer wires to ii, and I conclude from ii through the interposed acid and paper to copper 3, and back through that copper to the zinc 4 and so from it through the acid in i to the copper i. This might be expected from the expt. 1927, and shews that the electricity *could pass* between the coppers, the charge being Sulpho nitric acid and the electrophores not connected. This would be a losing current in the active battery.

1946. When, instead of touching the copper i, the wire *a* touched the zinc 4, or any part of its copper 3, a current equally strong, but in the reverse direction, was produced. This effect also might be expected, as the electrophores were still unconnected.

1947. When (the electrophores not being in contact), the wire *a* touched any zinc to the right of the inserted plate ii, or any copper in metallic contact with such zincs, or the zinc plate 4 or its copper, then the electric current was from the inserted plate ii to the galvanometer and away to the touched plate. But when *a*

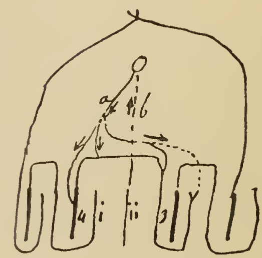
touched any zinc plate to the left of the zinc plate 4, or any copper in metallic connection with such zinc plates, the current was from the galvanometer to the inserted plate and so round to the plate touched by *a*.

1948. Now brought the electrophores in contact, so as to allow the battery current to circulate and relieve the tension of the apparatus. In this case, when *a* was communicated with the copper i, the current was as strong as in the former case when the electrophores were not in contact; but in the reverse direction, being from the inserted plate ii to the copper i. This current is in the same direction as that through the battery, and would rather aid than be a cause of loss of power; and at all events it goes to prove that if ii were away, electricity would not go from i through the acid to the contiguous copper 3, for it appears rather that electricity proceeds from 3 to ii, which is now only a part of i: the outer face, as it were, of it.



1949. It seems just now as if an insulation between the coppers i and 3 would do more harm than good, since when there is no insulation, a current passes from one to the other in the direction of that which is general to the whole connected battery.

1950. If, whilst the electrophores were still in contact, the wire *a* were made to touch the copper 3 or the zinc 4 connected with it, the current is always from the interposed plate ii to the Galvanometer. This however would imply that the electricity of the Zinc 4 could pass through i and the acid next to it to 3 or at least to ii, which in one case is made part of 3 and gives a current through the Galvanometer to it. Must examine all these effects by two interposed plates and see how they are related to each other.



1951. When the wire *a* is placed in contact with zinc plates far to the right or left of the inserted plate ii, then irregular effects take place, dependant I think in part on a Marianini state of plates induced by the previous current.

1952. Now ceased these experiments; removed all the old charge; washed out the trough; made a new charge of Sulphuric and nitric acid; connected a volta electrometer with the trough; put it into action and collected the Oxygen and hydrogen evolved from the instrument, i.e. the volta electrometer. It gave 44 cubical

inches of oxygen and hydrogen, which, without making any correction for temperature, pressure and moisture, is equal to about 5·68 grains of water decomposed.

1953. The whole charge, being still somewhat acid, was withdrawn, the plates washed and the liquid evaporated down to dryness, so as to give a dry uniform result; it weighed 5255 grains.

18 SEPT. 1834.

1954. 200 grains of this mass were dissolved in water—heated—excess of carb. soda added to it—the whole boiled—the precipitate well washed—filtered and dried on a warm plate. It gave 109 gr. carb. zinc; but on heating this in a platina crucible on the sand bath, water continued to rise and also carbonic acid; it was therefore heated red hot and became 79·57 grs. of oxide of zinc without any carbonate. The 5255 grs. contained therefore 2090 grs. of oxide of zinc or 1677 grains of zinc.

1955. Hence each of the 40 plates must have lost nearly 42 grains of zinc. Now 5·68 grains of water is equivalent to 20·5 grains of zinc, so that twice the quantity of zinc was oxidized and dissolved than was really needful in its most effective condition to produce the electricity required to decompose the water in the Volta electrometer.

1956. Still, the quantity of acid used for this decomposition was very small as compared to that required in the ordinary battery. The exhausted charge contained no copper. On acting on it by the carb. soda or by lime or alkali, much ammonia was found present, probably formed by the action of the Zinc, etc. on the N.A. of the charge.

1957. It is very probable that a dilute pure S.A., i.e. without N.A., will be a more economical charge as to the action on the zinc plates; must try it and get the results.

1958. Expts. 1944 and 1948 shew that when the wire *a* is touching the copper i, reverse effects are produced according as the electrophores are in contact or not at *x*. Proceeded to make communication at *x* by means of different fluids, to ascertain when the reversion would commence. If contact be metallic at *x*, the electric current is from the inserted plate ii to the galvanometer and away through the wire *a* to the copper i.

1959. On putting a volta electrometer filled with dilute Sul. Acid at x, the current was in the same direction. On dipping platina plates connected with the electrophores into my volta electrometer Sul. Acid, the current was still in the same direction, although the plates were 6 inches apart and did but just touch the acid.

1960. On dipping the same plates into a basin of water the current was reversed, being the same as if the electrophores were not communicated. Adding a little Sul. Soda, still retained the current in the same state. But adding a little Sul. Acid, the conducting power was so far increased that by putting the plates either far into the fluid or near together, the effect of connected Electrophores could be obtained; or by putting them farther apart or but little immersed the effect of non contact of the electrophores was produced.

1961. The charge used for these experiments contained a little copper, but it was that portion which had been acted upon at the time the battery was tuned up and out of action—flowing indeed from the interposed papers.

1962. Now dismounted the trough, took out all the papers and put in others of double cartridge paper which had been waxed, each having been dipped in a bath of yellow wax heated to 310°, so as to drive out air and water vapour and leave the papers thoroughly impregnated.

19 SEPTR. 1834.

1963. Used the waxed paper battery, 40 pr. 3 inch plates, the charge, as before, being Sul. N. Acid of the usual strength. The gas in the volta Electrometer was 37·8 c. inches of Ox. and Hy. = 4·88 grains of water decomposed.

1964. The charge, being removed, was made up to 140 c: i: uniformly mixed; and then 10 c: i: were put into a flask, made hot, sol. carb. soda gradually added until in excess, the whole boiled, the precipitate well washed, filtered, dried, ignited in a platina crucible and weighed: it was pure oxide of zinc and amounted to 103·7 grains. Hence the whole charge contained 1451·8 gr. = 1165 gr. metal zinc; and each of the 40 plates had lost 29·1 grains.

1965. Now 4·88 gr. of water are = 17·6 zinc, so that 1·65 times

the equivalent of zinc had been used to decompose the water. This is a good result, better than the one from the trough with only common paper. $1{\cdot}65 \times 40 = 66$, the number of equivalents of zinc in the whole battery used to decompose elsewhere one equivalent of water.

1966. Now used two porcellain troughs with Wollaston's plates, 4 inches square, the two containing 20 pr. plates. The acid was of the same charge as before. Here the action was far more steady and durable than before and for any given time more abundant, but then the zincs have nearly double the surface of the others and the acid is in much greater quantity.

1967. The quantity of gas from the Volta electrometer was 124·5 c: i: = 16·1 gr. of water decomposed.

1968. The charge mixed altogether = 480 c: i: Of this, 10 c: i: were decomposed as before and gave 160 grs. oxide. The 480 c: i: therefore contained 7680 grs. of oxide or 6237 grs. zinc, and each of the 20 zinc plates must have lost 311·8 grs. of metal. Now 16·1 of water are equivalent to 58·14 of zinc, so that 5·36 equivalents of zinc from each plate had been used to obtain one equivalent of action in the volta electrometer—$5{\cdot}36 \times 20 = 107{\cdot}2$, the whole number of equivalents of zinc used in the battery in this case.

1969. Used now only one Wollaston's trough, i.e. 10 pr. 4 inch plates, double coppers—same charge as before. The current of gas at the Volta electrometer was apparently much less than it ought to be, and from circumstances the acid within had been diluted about $\frac{1}{3}$ with water and was not therefore so good a conductor. The experiment therefore not fairly comparable with the former.

1970. The gas from the Volta Electrometer was 54 c: i: = 7 grains water. The whole charge was 240 c: i: 10 c: i: gave 222 grs. oxide: the 240 c: i: contained therefore 5328 grs. oxide or 4275·5 zinc, and each of the ten plates must have lost 427·55 gr. of zinc. The 7 gr. of water are equivalent to 25·28 zinc: so that 16·9 equivalents of zinc from each plate or 169 equivalents from the whole trough had been used to decompose one equivalent of water.

1971. Collected results of battery action in producing the decomposition of one equivalent of water in a volta electrometer:

	Equivalents of Zinc used.				at one plate.	at whole battery.
1952–1955	My 40 pr. 3 inch plates—double coppers—common papers				2·05	82·0
1963–1965	. . . . waxed papers				1·65	66·0
1966–1968	Two porcellain trough 20 pr. 4 inch—double coppers				5·36	107·2
1969–1970	One . . . . . 10 pr. Do. Do.				16·90	169·2
1996	My 40 pr. 3 inch—wax papers	W 200—	S. A. 9—	N. A. 4 vols.	2·786	111·5
1997	. . . . L. V. E.	200	9	8	2·260	90·4
1998	. . . . L. V. E.	200	9	0	4·392	175·7
2001	. . . . L. V. E.	200	0	8	1·854	74·16
2003	. . . . L. V. E.	200	0	16	1·82	72·80
2004	. . . . L. V. E.	200	0	32	2·10	84·0
2005	. . . . L. V. E.	200	$4\frac{1}{2}$	4	2·16	86·4
2006	. . . . S. V. E.	200	$4\frac{1}{2}$	4	2·17	86·8
2007	. . . . both V. E.	200	$4\frac{1}{2}$	4	1·207	48·28
2008	. . only 20 pr. . . . . . L. V. E.	200	$4\frac{1}{2}$	4	2·53	50·60
2009	. . only 10 pr. . . . . . L. V. E.	200	$4\frac{1}{2}$	4	3·725	37·25
2013	40 pr. 4 inch—Porcellain—Old—L. V. E.	200	$4\frac{1}{2}$	4	3·544	141·76
2015	20 pr. Do. Do. Do.	200	$4\frac{1}{2}$	4	5·25	105·00
2016	10 pr. Do. Do. Do.	200	$4\frac{1}{2}$	4	14	140·00
2017	My 20 pr. 4 inch—wax papers—New—L. V. E.	200	$4\frac{1}{2}$	4	2·32	46·40
2018	. . 10 . . . . L. V. E.	200	$4\frac{1}{2}$	4	6·758	67·58
2019	. . 20 . . . . both V. E.	200	$4\frac{1}{2}$	4	4·85	97·00
2020	. . 20 . . . . again as 2017	200	$4\frac{1}{2}$	4	4·47	89·40
2027	20 pr. 4 inch—Porcellain—New 1st time	200	$4\frac{1}{2}$	4	3·7	74
2028	My 40 pr. 3 inch—common papers—no glasses	200	$4\frac{1}{2}$	4	2·527	101
2029	. . 20 pr. 4 inch Do. Old	200	$4\frac{1}{2}$	4	3·864	77·28
2039	. . 20 pr. 4 inch—porcellain—used 3 or 4 times	200	$4\frac{1}{2}$	4	5·9	118
2040	. . 10 pr. 4 inch—porcellain (double acid)	200	9	8	24·435	244·35
2041	. . 10 pr. 4 by 8—porcellain (single acid)	200	$4\frac{1}{2}$	4	25·68	256·8
2043	. . 20 pr. 4 inch—porcellain	200	$4\frac{1}{2}$	4	14·43	288·6
2044	. . 40 pr. 4 inch—porcellain (half acid)	200	$2\frac{1}{4}$	2	4·7	188
2045	. . 20 pr. 4 by 8—porcellain (half acid)	200	$2\frac{1}{4}$	2	6·54	130·8
2046	. . 10 pr. 8 by 8—porcellain (half acid)	200	$2\frac{1}{4}$	2	16·4	164·0
2047	My 20 pr. 4 inch—common paper (single acid)	200	$4\frac{1}{2}$	4	3·57	71·4
2054	. . 20 pr. 4 inch—porcellain (have rested)	200	$4\frac{1}{2}$	4	9	180
2055	My 20 pr. 4 inch—common paper (Do.)	200	$4\frac{1}{2}$	4	3·265	65·3
2056	My 20 pr. 4 inch—Wax paper cells	200	$4\frac{1}{2}$	4	3·275	65·5

May take 100 c: i: of damp O. and H. at 60° and 30 inches bar. as = 12·68 gr. of water.

1972. With regard to the superiority of many plates over few plates in decomposing power, not merely by comparison of one plate from each arrangement, but of the *whole* of the metal in one battery with the *whole* of that in another, it of course must depend on the intensity obtained by many plates; for that intensity urges the current round through the decomposing or bad conductor; and in proportion as that is facilitated the proportion of chemical action which becomes *current* instead of *local* is increased, and of course there is less lost force.

1973. With a perfectly good conductor nearly all the electricity is passed, i.e. *nearly all* the chemical power becomes *current* even with a single pair of plates. With an interposed non conductor NONE of the chemical power becomes current. With an imperfect conductor more or less of the chemical power becomes *current*, as the circumstances favouring the conducting power across the imperfect conductor are exalted or diminished. These circumstances are

Actual increase or improvement of the conducting power.
Enlargement of the Electrodes.
Approximation of the Electrodes.
Increased intensity of the passing current.

1974. The appearance of hydrogen or gas rising from the zincs in the trough is bad. In a trough unconnected in which it was thus rising, when the ends were connected by a thick wire much less rose from the zinc and more from the neighbouring coppers. This shews that making the communication better or perfect converts *local* into *current* chemical action.

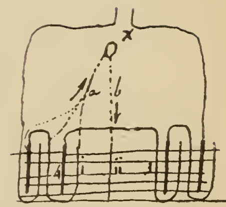
1975. No hydrogen ought to rise from zincs in a perfect and connected trough even though the charge Sul. Acid only. But as there are impurities in the zinc there are always a number of small circles which will cause such evolution.

1976. Pure zinc, if it could be had, would be very important in the construction of a battery, because it would prevent these small circuits. Could it not easily be redistilled at the Zinc works?

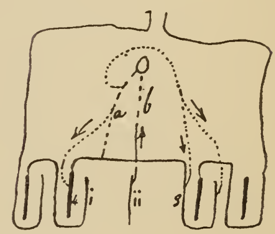
1977. There was no copper in any of the last three experiments in the charge of the troughs; zinc only had been dissolved. The way in which copper gets into the charge is by the latter dissolving the dirty deposit of copper always formed from common zinc

because of its impurities, this solution taking place when the plates are removed from the acid. When immersed, a little of this copper precipitates on the zinc by *local* chemical action, and then produces with it small circuits occasioning sad loss of power.

1978. *Volta electrometer.* The plates should not be so close that, when there is great evolution of gas between them, it may, by not getting away quickly enough, retard conducting power. The wires should be thick, or else they become heated, and even ignited, and then crack the glass. The plates should be larger than I have made them—at least for the discharge of the current of a battery.

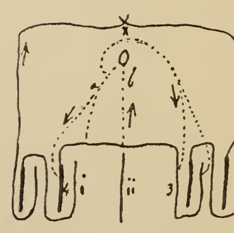


1979. Experimented upon the condition of the interposed copper plate (1944) with the trough of 40 pr. 3 inch plates, which had now Waxed papers interposed (1962), instead of unwaxed papers. When the wire *a* touched the copper i in any part, or the zinc plate attached to [it] (the circuit of the battery not being closed at *x*), the current was from the galvanometer to the interposed plate *b*, just as if the papers had been unwaxed (1944).



When the wire *a* (the circuit of the battery not being completed) touched the zinc 4 or any part of the copper 3 connected with it, then the current was from the interposed plate to the galvanometer and back through the wire *a*, just as when unwaxed paper were used in the battery (1946).

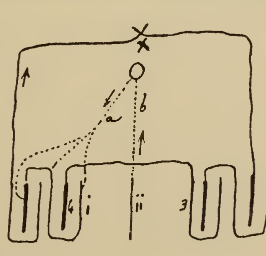
1980*. When the battery circuit at *x* was *complete* and the wire *a* was made to touch the copper i or its zinc, the current was from the inserted plate ii to the galvanometer and back through the wire *a*, as in the battery without the waxed paper (1948), and therefore the reverse of what takes place with the battery unconnected at *x*. This is the important condition of things, since when the battery is in use it is always more or less connected at *x*.



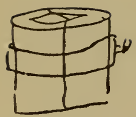
1981. When the battery circuit at *x* was *complete*, if the wire *a* touched the zinc 4 or any part of the copper attached to it, the current was from the interposed plate ii to the Galvanometer and back through *a* to the zinc 4: but it was weaker than in the case of unwaxed papers (1950), or in the case of non-contact at *x* (1946, 1979).

1982. When the wire *a* touched the zinc 4 or its copper (the electrophores not in contact), or any zinc or copper to the right

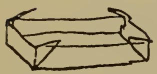
* [1980]



of the interposed plate, the current was from that plate to the galvanometer and away through wire *a*. But when it touched any zinc to the left except 4, or any copper of such zincs, then the current was from the wire *a* to the galvanometer and so to the interposed plate, just as when unwaxed papers were used. See (1947).



1983. These effects appear to shew that waxed paper conducts like unwaxed paper, though not so strongly. Put (to try this) a diaphragm of waxed paper across my bisected cell and put poles from a battery on each side. There was action as if conduction through the waxed paper.



1984. Believing this to be due to the little film of acid which could creep by the paper edges and cause moistening, I folded up a paper dish, and floating it on dilute S. A., put some also inside, and then put one pole outside and the other in. There was now no conduction and hence wax does insulate.

1985. But it is evident how much a little contact allows, and I believe that wet ordinary porcellain partitions are of very little consequence. See 1986 in correction as to insulation.

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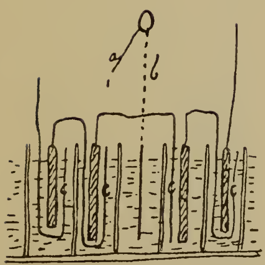
1986. Experimented on insulating power of divisions in a porcellain trough. When a zinc and a copper plate were put into two neighbouring cells and connected by a galvanometer, the division of porcellain between them being also dry at the upper edge, there was scarcely any current. The insulation was very good.

1987. When the trough was tilted so that the acid in the two cells communicated over the edge a little at the lowest side, there was conduction and deflection.

1988. The putting in of too much acid, or the increase in bulk of the liquid from the gas rising in a charged trough, often does as much or more than was done in the preceeding expt.



1989. Repeated the experiments with an interposed plate (1944, 1979, etc. etc.), using now the porcellain trough and insulating the interposed plate by porcellain divisions on both sides, thus. Deflections of the galvanometer needle occurred as in the former cases, and in the same directions in all cases; but they were to a very small extent. If the divisions had insulated perfectly there

would have been none. If the acid had flowed over the top of the partitions there would have been more.

1990. In the expts. with waxed papers, the influence gets round the sides and bottoms of the papers, for the paper itself does not conduct (1984).

1991. In the unwaxed papers (1944) the effect is a maximum.

1992. The simple question which needs practically to be settled is whether it is favourable or disadvantageous *when the battery is connected in a circuit.*

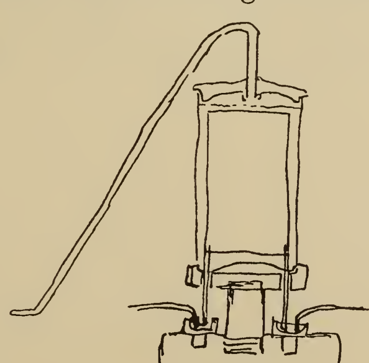
1993*. Have made a new Volta Electrometer with large plates and thick wires. The jar was   inches deep and   inches in diameter: the plates 4 inches high and 2·3 inches wide. The acid in the jar covered them. The thick platina wires soldered to them by gold passed through the bottom into ivory cups containing mercury, and here the communication was made. The liquid in the jar was diluted Sul. Acid of S. G. 1·336. The top of the jar was of turned mahogany, which had been soaked in wax at the temp. of 300° and allowed to cool under the surface of the wax. It was perfectly tight and answered exceedingly well.

1994. Proceeded to experiment upon my trough and others for the purpose of comparison, using this new volta electrometer to measure the action, and exciting the trough by acids of known strength. For this purpose Sulphuric and nitric acid of the same constant strength was used. The sulphuric acid was strong oil of vitriol. The nitric acid was of the ordinary kind and a cubical inch of it dissolved   grains of marble.

1995. Besides this, the quantity of charge was always ascertained and a portion of it analysed by being precipitated by carb. soda in excess, boiled, decanted, washed, dried, heated red hot and the proportion of oxide of zinc left ascertained.

1996. i. *My trough of* 40 *pr.* 3 *inch plates*, *double coppers*, *wax papers*. Proportions of the Charge by Vol., 200 W.; 9 S.A.; 4 N.A. The connection made with the volta electrometer before the charge was poured on to the plates and continued until the action was very slow and almost nothing. The acid was poured

* [1993]



back once or twice to mix it and returned. At the last it came off tasting neutral or nearly so, but sulphate of zinc soon covers the taste of acid. The acid evolved a little gas from the plates in the trough. After the action had ceased the acid was withdrawn, estimated and analysed. The plates were washed in clean water and were then ready for a second experiment. This was the course pursued in all. In the present instance—

The O. and H. from the Volta Electrometer (large one) was 28·5 cubic inches = 3·614 grs. of water decomposed.

The quantity of charge was 134 c: i: Of this, 10 c: i: gave 135 gr. of oxide of zinc. The whole charge contained therefore 1809 Ox. Zinc, or 1451·6 gr. metal, and each plate had lost nearly 36·3 grs. zinc. Now 3·614 water are equivalent to 13·05 grs. of zinc, so that 2·786 equivalents of zinc from each plate had been used, or 111·5 equivalents from the whole battery.

1997. ii. *My trough of* 40 *pr.* 3 *inch plates, double coppers, waxed papers.* Charge, W. 200; S.A. 9; N.A. 8 vols.; the N.A. double that in i, rest unchanged.

The action here was strong, the heat in the trough great and much waxy scum floated up, from the paper, melted by the heat, displaced by the gas and currents, etc.

The O. and H. from large volta electrometer = 39 c: i: = 4·945 gr. water decompd.

The vol. of charge was 135 c: i:; 10 c: i: gave 149 gr. ox. zinc; the whole charge contained therefore 2011·5 gr. Ox. Zinc or 1614·16 gr. Zinc. Each plate had lost 40·354 gr. zinc. As 4·945 W. are equivalent to 17·857 gr. zinc, each plate had lost 2·26 equivalents for one equiv. of water, or the battery had lost 90·4 equivalents.

1998. iii. *My trough of* 40 *pr.* 3 *inch plates, double coppers, waxed papers.* Charge, W. 200; S.A. 9; N.A. none; to compare with i and ii in that respect. There was much gas of course evolved here from the battery plates. It seemed to be irregular, more rising between some plates than between others. The gas, by swelling the bulk of the acid, made it overflow the edges of the plates, and so add to the discharging effect (if any) round the edges and bottoms. The heat produced in the battery was very little compared to iii or even ii.

The O. and H. from Volt. Electr. was 16·5 c: i: = 2·092 grs. of water decomposed.

The vol. of charge was 139 c: i:; of which 10 c: i: gave 119 grs. Ox. Zinc, or 1654 grs. for the whole charge = 1327·3 zinc or 33·18 gr. each plate loss of metal.

As 2·092 water are equivalent to 7·554 zinc, each plate had lost 4·392 equivalents, or the whole battery had lost 175·7 equivalents of zinc.

1999. When the charge is dilute S.A. without N.A., much copper in black flakes, loose, is thrown up. This I presume is the impurity of the zinc which is loosened by the hydrogen gas acting mechanically and tearing it off from the surface, where the charge acting without evolving gas would leave it. As such particles tend to discharge the circuit on the surface of the zinc, their removal is important and the use of dilute S.A. now and then on the plates may be very valuable in effecting this object. Can try this in a glass with a single pair.

2000. It is important to keep all copper in solution and also all loose copper out of the trough. The lead in the zinc would probably do no harm because of its electric relation to it.

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2001. iv. *My trough* 40 *pr.* 3 *inch plates, double coppers, waxed papers.* Charge, W. 200; N.A. 8; S.A. none—to compare with i, ii and iii. The action was good at first but soon fell. As there was no gas evolved in the trough, there was no mixing action there, and the charge had to be transferred to and fro several times. The acid soon tasted neutral—there was no heat scarcely evolved in the battery—the exhausted charge was very clean and brought away no loose copper or lead flocculi or particles.

The gas from V. Electr. (large) was 11·5 c: i: = 1·458 grs. water decomposed.

The vol. of charge was 139 c: i:; of these, 10 c: i: gave 35 gr. ox. zinc, and the 139 contained therefore 486·5 grs. Ox. zinc, or 390·4 gr. metal zinc; or each plate had lost 9·76 grains.

As 1·458 W. is equivalent to 5·265 zinc, each plate had lost 1·854 equivalents, or the whole battery 74·16 equivalents.

2002. A portion of this exhausted charge being put aside (as the

rest were), bubbles of gas rose from it for some time afterwards.

2003. v. *My trough* 40 *pr.* 3 *inch plates*, *double coppers*, *waxed papers*. Charge, W. 200; N.A. 16; S.A. none—to compare with former. The action was good and quick—slackening suddenly rather—there was no copper or lead loosened—no copper in solution, or at least very little. After the exhausted charge was removed and put aside, little bubbles of gas rose from it as in the last case.

The gas from large volt. electr. = 23·2 c: i: = 2·942 grains water decomposed.

The vol. of charge was 144 c: i:; of these, 10 c: i: gave 67 gr. oxide of zinc; the whole contained therefore 964·8 gr. Ox. Zinc or 774·2 gr. metal zinc; or 19·355 gr. loss each plate.

As 2·942 W. are equivalent to 10·624 Z., each plate had lost 1·82 equivalents, or the whole had lost 72·8 equivalents of Zinc.

2004. vi. *My trough of* 40 *pr.* 3 *inch plates*, *double coppers*, *wax papers*. Charge, W. 200; N.A. 32; no S.A.—to compare with the former. The gas in trough was strong—much heat evolved there—some gas set free in the trough—query what gas—action fell quickly at the close. Whenever N.A. is used in the charge Ammonia is formed—of course at the cathodes in the cells.

The gas evolved in large V. Electr. = 38·5 c: i: = 4·882 gr. water decomposed.

The vol. of charge was 140 c: i:; of which 10 c: i: gave 132 gr. Ox. Zinc and the whole therefore contained 1848 gr. ox. zinc or 1483 gr. metal zinc. Each plate had lost therefore 37·075 gr. metal.

As 4·882 W. is equivalent to 17·63 zinc, each plate had lost 2·1 equivalents or the whole battery 84 equivalents of zinc.

2005. vii. *My trough of* 40 *pr. plates* 3 *inches*, *double coppers*, *wax papers*. Charge, W. 200; S.A. 4·5; N.A. 4 vols. Acid half strength of ii. Action very fair and steady.

The gas evolved from large V. Electr. = 23·3 c: i: = 2·954 gr. water decompd.

The charge = 140 c: i:; of which, 10 c: i: gave 82 gr. and the whole therefore 1148 gr. oxide of zinc, or 921·2 gr. metal zinc = 23·03 gr. zinc loss each plate.

As 2·954 W. is equivalent to 10·667 zinc, so each plate had lost 2·16 equivalents, or the whole battery 86·4 equivalents.

2006. viii. *My trough of* 40 *pr.* 3 *inch plates, double coppers, wax papers.* Charge, W. 200; S.A. 4·5; N.A. 4. Charge the same as vii, but now used my Volta electrometer with small Electrodes (   ) instead of the one with large plates; but the acid in both was the same. The action went on well, but this volta electrometer and its contents soon became warm.

The gas from V. Electr. = 22·75 c: i: = 2·885 gr. water decomposed.

The charge = 140 c: i:; of this, 10 c: i: gave 80·5, and the whole therefore 1127 gr. oxide of zinc or 904·38 gr. metal zinc: each plate had lost therefore 22·61 grains of zinc.

As 2·885 W. are equivalent to 10·418 zinc, so each plate has lost 2·17 equivalents—or the whole battery 86·8 equivalents of zinc.

Is very beautiful as compared with vii, and shews equality of large and small electrodes very well.

2007. ix. *My trough of* 40 *pr.* 3 *inch plates, double coppers, wax papers.* Charge, W. 200; S.A. 4·5; N.A. 4. Charge the same as vii and viii, but now used both V. Electrometers, connecting them in succession and collecting the gas from each separately. The action in the battery and instruments went on very well and there was little apparent retardation.

The gas from large volt. Electr. = 21 c: i: = 2·6628 gr. water decomposed.

The gas from small V. E. = 21 c: i: = 2·6628 gr. water decomposed.

The charge = 140 c: i:; of this, 10 c: i: gave 83 gr., and the 140 c: i: therefore 1162 gr. of oxide or 932·5 metal zinc = 23·31 loss each zinc plate.

As 2·6628 W. is equivalent to 9·615 zinc, so each plate had lost 2·414 equivalents and the whole battery had lost 96·56 equivalents as compared with the decomposition in one V. Electrometer.

But as compared with all the water decomposed, the loss of each plate is only 1·207 equivalents, or of the whole of the zinc used, 48·28 equivalents.

2008. x. *My trough of* 40 *pr.* 3 *inch plates*, *double coppers*, *wax papers*; but arranged it as in the diagram, with a wire *a* connecting the 20th zinc plate with the negative end of the battery, so that, though the whole battery was charged, only the power of 20 pr. of plates was active at the Volta electrometer.

Charge, W. 200; S.A. 4·5; N.A. 4 vols., or same as vii, viii and ix. The action at the V. Electr. was very fair and good.

Gas in large Volta electrometer = 20 c:i: = 2·536 gr. water decomposed.

Whole charge 140 c:i:; of which, 10 c:i: gave 82·5 gr. and the 140 c:i: therefore 1155 gr. oxide of zinc or 926·8 metal zinc. Each plate therefore had lost 23·17 gr. zinc.

As 2·536 W. are equivalent to 9·16 zinc, so each plate has lost 2·53 equivalents, or the 20 active plates 50·6 equivalents.

2009. xi. *My trough of* 40 *pr.* 3 *inch*, *double coppers*, *wax papers*—but arranged as above, so as to employ only 10 pr. of plates in decomposing.

Charge, W. 200; S.A. 4·5; N.A. 4 vols., as in vii, viii, ix and x. There was much less action in Volta electrometer now.

Gas in large V. Electr. 13·5 c:i: = 1·712 gr. water decomposed.

Whole charge 140 c:i:; of which, 10 c:i: gave 82 gr., or the whole 1148 gr. of oxide or 921·2 gr. metal zinc. Each plate lost therefore 23·03 gr.

As 1·712 W. is equivalent to 6·1822 zinc, each plate had lost 3·725 equivalents or the whole ten in use, *37·25 equivalents*.

2010. What is the state of the connecting wire *a* in *x*? Must think of and examine this point WELL.

2011. Refer to Gay Lussac and Thenard, Researches Electro chemiques, etc. for their battery results.

2012. Make a galvanometer with a descending arm passing into water to stop the Vibrations. Will settle well and quickly. *Good.*

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2013. xii. 40 *Pr.* 4 *inch plates*, *double coppers*, *porcellain troughs*, *etc.* Charge, W. 200; S.A. 4·5; N.A. 4. The plates were old and some of them even perforated. But they had been acted on slightly by acid and brushed and made clean from all adhering dirt or metal. The action was continued for 30′ and was regular and good

the greater part of the time. The plates were raised once or twice to mix the acid.

Gas by large V. Electr. 78·5 c: i: = 9·954 gr. water decomposed.

Whole charge 1000 c: i:; of these, 10 c: i: gave 63·5 gr. and the 1000 there 6350 grs. of oxide, or 5095·68 metal zinc. Each plate had lost therefore 127·4 gr. of zinc nearly.

As 9·954 W. is equivalent to 35·945 Z., each plate has lost 3·544 equivalents; or the whole battery 141·76 equivalents.

2014. In these plates the coppers and zincs are farther apart at the acting surfaces than in mine: this, by lengthening the bad conductor, opposes the *current* action and increases the *local* action—especially when the copper or lead on the surface of the zinc accumulates, so as to form a serious number of discharging voltaic circuits, making no part of the whole, but diminishing its power.

2015. xiii. 20 *Pr.* 4 *inch plates*, *double coppers*, *porcellain troughs.* Zincs had been used but were good and clean.

Charge, W. 200; S.A. 4·5; N.A. 4. Allowed to act half an hour; the action was good.

Gas in large V. Electr. 53·4 c: i: = 6·77 gr. water decomposed.

Whole charge 500 c: i:; 10 c: i: gave 64 gr. and the 500 therefore 3200 gr. oxide or 2568 gr. metal zinc = pr. plate to 128·4 gr. zn.

As 6·77 W. is equivalent to 24·447 Z., so each plate has lost 5·25 equivalents or the whole 20 pr. 105 equivalents of zinc.

2016. xiv. 10 *Pr.* 4 *inch plates*, *double coppers*, *porcellain troughs*; old but clean good plates. Charge, W. 200; S.A. 4·5; N.A. 4. Action for half an hour.

Gas in large V.E., 20 c: i: scarcely = 2·536 gr. water decomposed.

Whole charge 250 c: i:, of which, 10 c: i: gave 64 gr. and the whole therefore 1600 gr. oxide or 1284 gr. metal zinc; each plate had lost therefore 128·4 gr. zinc.

As 2·536 W. is equivalent to 9·16 Z. nearly, so each plate had lost 14 equivalents, or the whole ten 140 equivalents of zinc.

2017. xv. *My* 20 *pr.* 4 *inch plates*, *double coppers*, *waxed papers*; used for the *first time*, in round trough. Were dipped in and out at required times of action, etc. etc.

Charge, W. 200; S.A. 4·5; N.A. 4. Action continued 20′; was very good action, bubbles of gas the largest we have had yet. Stirred the acid three times during the 20′; renewal of action very considerable each time.

Gas in V. E. 70·4 c: i: = 8·927 gr. of water decomposed.

Whole charge 270 c: i:; 10 c: i: gave 69 gr. oxide and the whole charge therefore 1863 gr. oxide or 1495 gr. metal zinc = each plate had lost therefore 74·75 gr. zinc.

As 8·927 W. is equivalent to 32·2364 Z., so each plate has lost 2·32 equivalents nearly, or the 20 pr. plates 46·4 equivalents of Zinc.

2018. xvi. *My* 20 *pr.* 4 *inch*, *double copper*, *wax papers*. 10 pr. only used as in xi.

Charge, W. 200; S.A. 4·5; N.A. 4. Acted for 30′; action slow by comparison with xv.

Gas in V. E. 23·1 c: i: = 2·93 gr. water decomposed.

Whole charge was 270 c: i:; 10 c: i: gave 66 gr. and 270 c: i: 1782 gr. of oxide or 1430 metal zinc = 71·5 for each of the plates.

As 2·93 W. is equivalent to 10·58 Z., so each plate had lost 6·758 equivalents, or the ten pair used 67·58 equivalents.

2019. xvii. *My* 20 *pr.* 4 *inch plates*, *double coppers*, *used with both V. E. as ix*. Charge, W. 200; S.A. 4·5; N.A. 4. Action very slow; continued it for 20′.

Gas in large V. E. = 14·5 c: i: = 1·8386 gr. water decomposed.
„ small V. E. = 14·6 c: i: = 1·85 gr. water decomposed.

Whole charge 270 c: i:; 10 c: i: gave 59·5 gr. and 270 c: i: gave therefore 1606·5 gr. of oxide, or 1289 gr. metal zinc. Each plate had lost therefore 64·455 gr. metal.

As 1·84 W. is equivalent to 6·644 Z., so each plate had lost 9·7 equivalents, or the whole battery 194 equivalents, for the water decomposed at either V. E., or the half of those quantities as the equivalent of the water decomposed at *both V. E.*

2020. xviii. *My* 20 *pr.* 4 *inch double coppers*, *wax papers*, tried again as in xv, this being now the 4*th* time of using instead of the *first*.

Charge, W. 200; S.A. 4·5; N.A. 4. Action continued 20′; was poor as compared to xv. A few floculli of lead and copper, etc. came off into the acid during the action, and after action or rubbing

the plates of Zinc with a feather only, much of such metal came off. This had done the harm.

Gas in large V. E. 36·5 c: i: = 4·628 gr. water decomposed.

The whole charge was 270 c: i:; 10 c: i: gave 69 gr. oxide, or the whole 1863 oxide or 1495 gr. metal zinc = each plate had lost therefore 74·75 gr. zinc.

As 4·628 W. is equivalent to 16·71 Z., so each plate had now lost 4·47 equivalents or the whole battery 89·4 equivalents.

This is nearly twice what it lost before. Proof of value of new plates, i.e. of plates free superficially from copper. Must try it again hereafter. Must try also 20 new Porcellain sets of plates.

2021. One cubic inch of the Nitric acid used to excite the batteries in these experiments can dissolve just 150 grains of marble. There is scarcely a trace of Sulphuric or Muriatic acid in it.

2022. Must try the two kinds of zinc used in cast and rolled plates.

2023. Took the 40 pr. 3 inch plate battery to pieces and examined it. The wax was out of the papers and they were wet and soaked with solution of zinc. The wax was about the plates. In this case it had done no final good, but harm in dirtying the plates, etc.

2024. The zincs were much eaten away—pretty regularly, but of course rough, and a powder of lead and copper particles upon them. The whole 40 pr. of zinc and copper weigh 124 oz., hence 10 pr. would weigh 31 oz. Ten pair of new ones weighed 54 oz., so that the ten old zincs had lost 23 oz., or each zinc plate had lost 2·3 oz. According to the notes, about this quantity should have been used.

2ND OCTR. 1834.

2025. Have repacked the three inch plates, old as they were; have used double cartridge papers, unwaxed; have taken out the side glasses and put in a piece of board instead, so that, as to insulation, no care is now taken. But I think that, with this form of plate, side glasses (of crown glass) would be useful.

2026. Now proceeded to experiment still further on the amount of action from different forms of voltaic instruments.

2027. xix. *Twenty pair, 4 inch plates. New zincs and coppers; first time using.* Porcellain troughs.

Charge, W. 200; S. A. 4·5; N. A. 4. Action continued for 50′. After the action there was much black powder on the zinc plates.

Gas in Large V. E. 89 c: i: = 11·3 gr. water decomposed nearly.

Whole charge 460 c: i:; of these, 10 c: i: gave 81·7 gr., and the 460 c: i: 3758 grains therefore of oxide of zinc, or 3015·7 gr. zinc = 150·8 gr. per plate loss of metal.

As 11·3 W. is equivalent to 40·8 Z., so each plate has lost 3·7 equivalents nearly or the whole 20 plates 74 equivalents.

2028. xx. *My* 40 *pr.* 3 *inch*, *old*, *common papers*, *no glasses*.

Charge, W. 200; S. A. 4·5; N. A. 4. Action quick—continued for 25′.

Gas in Large V. E. 24·5 c: i: = 3·1 gr. water decomposed.

Whole charge 150 c: i:; 10 c: i: gave 94 gr. oxide, so the 150 c: i: contained 1410 gr. of oxide or 1131·4 of zinc = 28·3 loss per plate.

As 3·1 of water is equivalent to 11·2 Z. nearly, so each plate had lost 2·527 equivalents, or the whole 40 pr. 101 equivalents of Zinc.

2029. xxi. *My* 20 *pr.* 4 *inch*, *common papers*, *old in use*.

Charge, W. 200; S. A. 4·5; N. A. 4. Acts well; time about 30′, but not noted.

Gas in V. E. 52·25 c: i: = 6·625 gr. water decomposed.

Whole charge 300 c: i:; of these, 10 c: i: gave 76·8 gr. and the 300 c: i: 2304 gr. oxide = 1849 Zinc. Each plate had lost therefore 92·45 gr. Zinc.

As 6·625 water is equivalent to 23·924 zinc, so each plate has lost 3·864 equivalents or the whole 20 pr. 77·28 equivalents.

2030. I think electricity is lost at the edges with this form: better have the other form plates. No glasses at the bottom, but crown glass at the sides and perhaps ends.

2031. The use of Wollaston's double coppers very evident—simply depends on the circumstance that the action depends on the extent of surfaces of the electrolyte which is between the zinc and copper plates, and that this way the surfaces are the *largest* and the distance which the current has to travel through a bad conductor the *least* of what they can be in any other form. Hare's large calorimotor is effective as an arrangement on the same principle.

2032. Had two pieces of zinc of exactly equal surface. One was of cast zinc such as used in the porcellain troughs, and weig[h]ed 430 grains, being thicker than the other, which was of rolled zinc, such as I have used in my arrangements and which weighed 304·8 grains. An acid was mixed, W. 200; S. A. 4·5; N. A. 4, of which 6 c: i: was put into each of two vessels exactly alike in form. After some hours they were examined. There was black powder on both, but more on the rolled than the cast zinc. The acid of the cast zinc was not so far neutralized as that of the rolled zinc; the rolled zinc had lost 43·2 gr., the cast zinc only 36 grains. The cast zinc, having contained more impurity, had more of the black matter on its surface; and though this would increase voltaic action and increase rapidity of consumption, yet by retaining the fluid on the surface of the zinc, it prevented the exhausted acid being replaced by fresh and so diminished the action in a given time.

2033. There was little or no gas evolved from this acid to change the place of fluid. Probably with dilute S. A. only the action on the cast zinc would be quickest.

2034. The powder from the cast plates porcellain troughs is principally lead, with a good deal of zinc, some copper and some iron. The metal is foreign, as Newman tells me, and was new, i.e. from the ingot.

2035. If it were distilled it would be better. Zinc which, dissolving clean, also dissolves the slowest, ought to be the best.

2036. A piece of the fuzed zinc, when coated in powder, etc., is less effective than pure zinc when put into dilute acid with a piece of platina in contact with it.

2037. Prepared two troughs for 20 pr. 4 inch plates, porcellain troughs. Prepared also the round trough for my 20 pr. 4 inch plates. Charge in both, W. 200; S. A. 4·5; N. A. 4. Then compared the power in the best way I could, both on platina wire in tube and by spark between same charcoal points. On the whole mine quite equal to the porcellains, and perhaps even better. Thus as to spark and good conductors, as well as in decompositions, this form appears quite as good as the former.

2038. From the expts. of this day I begin to suspect that rest is useful in a battery in letting down that contrary state (Maria[ni]ni?) which they acquire by use, and which in action creates a reaction. Query is it possible that my form, by allowing a little discharge, can so reduce this opposed state and actually make the whole effect better. It looks a little like it.

2039. xxii. 20 *pr.* 4 *inch plates. Porcellain troughs.* Were used three or four times yesterday; are old therefore, but have rested.

Charge, W. 200; S.A. 4·5; N.A. 4. Acted for about half an hour.

Gas in Large V. E., 45·25 c: i: = 5·738 gr. water decomposed.

Whole charge 500 c: i:; of these, 10 c: i: gave 61 gr., and the whole therefore 3050 gr. oxide or 2447·5 gr. metal. Loss each plate 122·37 gr. Z.

As 5·738 W. is equivalent to 20·72 Z., each plate must have lost 5·9 equivalent or the 20 pr. 118 equivalents.

2040. xxiii. 10 *pr.* 4 *inch plates*, *porcellains*, with double acid. Charge, W. 200; S.A. 9; N.A. 8. Action on about half an hour; there was but little gas from V. Electrometer, but much at the plates in the cells. Great heat in the trough; much black flocculi of lead, copper, etc.

Gas in V. E. 27·8 c: i:, equal to 3·525 gr. water decomposed.

Whole charge 250 c: i:; 5 c: i: gave 77·5 gr., and the 250 c: i: 3875 gr. of oxide or 3109·5 gr. metal Zinc. Loss each plate 310·95 Zinc.

As 3·525 W. is equivalent to 12·73 Zinc, so each plate had lost 24·435 equivalents, or the whole ten 244·35 equivalents of zinc.

2041. xxiv. 20 *pr.* 4 *inch*, *porcellains*, *with single acid*, *in two tens.* The zincs had been scrubbed clean from powder, etc.

Charge, W. 200; S.A. 4·5; N.A. 4. In action for 35′; fair.

Gas in Large V. E. 23·2 c: i: = 2·942 gr. water decomposed.

Whole charge 500 c: i:; of these, 10 c: i: gave 68 gr. and the 500 c: i: therefore 3400 gr. oxide, or 2728·4 metal zinc. Loss per plate 136·42 gr. Z.

As 2·942 W. is equivalent to 10·624 Z., so each plate has lost 12·84 equivalents, or the 20 pr. 256·8 equivalents.

2042. On breaking and renewing the contact with one of the two troughs, the quantity at the V. Electrometer (of action)

varied in proportions approaching to 1 and 2, shewing that, the intensity remaining the same, if the size of the plates was enlarged, more electricity could pass a given decomposing obstructor.

2043. xxv. 20 *pr.* 4 *inch plates*, *porcellains*, *scrubbed clean.* Have been used yesterday and to-day—to compare with xxii.

Charge, W. 200; S. A. 4·5; N. A. 4. Acted about half an hour.

Gas in V. E. 21·25 c: i: = 2·694 gr. water decomposed.

Whole charge 500 c: i:; 10 c: i: gave 70 gr. oxide and the 500 c: i: gave therefore 3500 gr. oxide, or 2808·6 metal. Each plate lost 140·43 gr. Z.

As 2·694 W. is equivalent to 9·728 Z., so each plate had lost 14·43 equivalents, or the 20 pr. 288·6 equivalents.

2044. xxvi. 40 *pr. plates* 4 *inch*, *porcellains*, *end on*, *half acid*, *all clean.*

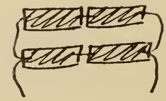
Charge, W. 200; S. A. 2·25; N. A. 2. Action continued for 30′.

Gas in V. E. 28 c: i: = 3·55 gr. water decomposed.

Whole charge 1000 c: i:; 20 c: i: gave 60 gr. and the 1000 therefore 3000 gr. oxide, or 2407·4 gr. metal Z. Each plate had lost 60·2 gr. Z.

As 3·55 W. is equivalent to 12·82 Z., so each plate had lost 4·7 equivalents, or the whole 40 pr. plates 188 equivalents.

2045. xxvii. 40 *pr.* 4 *inch plates*, *Porcellains*, *half acid. Zincs not cleaned; two and two, thus.*



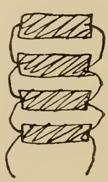
Charge, W. 200; S. A. 2·25; N. A. 2. The action was continued for 30′.

Gas in V. E. 37·5 c: i: = 4·755 gr. water decomposed.

Whole charge 1000 c: i:; 20 c: i: gave 56 gr. and the 1000 c: i: therefore 2800 gr. oxide of zinc = 2246·9 gr. metal zinc. Each plate had lost therefore 56·173 gr. zinc.

As 4·755 W. is equivalent to 17·17 Z., each plate has lost therefore 3·27 equivalents, or the 40 plates 130·8 equivalents.

2046. xxviii. *The* 40 *pr.* 4 *inch*, *porcellains*, *cleaned*, *half acid.* Arranged side by side, thus.



Charge, W. 200; S. A. 2·25; N. A. 2. Action continued for 35′.

Gas, 28·75 c: i: = 3·6455 gr. water decomposed.

Whole charge 1000 c: i:; 20 c: i: gave 54 gr. oxide. The 1000 therefore contained 2700 gr. oxide or 2166·66 metal zinc. Each plate had lost therefore 54·166 gr. metal.

As 3·6455 W. is equivalent to 13·1643 Z., so each plate has lost 4·1 equivalents, or the 40 plates 164 equivalents.

2047. xxix. *My* 20 *pr.* 4 *inch*, *common papers*, *standard acid*, *etc. again.* To compare with xxi, etc.

Charge, W. 200; S.A. 4·5; N.A. 4. Action continued for 30′. Gas in L. V. E., 56·7 c: i: = 7·19 grains of water decomposed.

Charge 300 c: i:; 10 c: i: gave 77 gr. and the 300 therefore 2310 gr. oxide or 1853·7 gr. metal zinc. Each plate had lost therefore 92·68 gr. metal.

As 7·19 W. is equivalent to 25·964 Z., so each plate has lost 3·57 equivalents nearly, or the 20 plates 71·4 equivalents.

2048. Used 20 pr. 4 inch porcellains with the standard acid, but made the electric current pass, not only through the large Volta electrometer, but also through *common river water*, by two platina plates each 3 inches long by $\frac{7}{8}$ wide and placed not quite $\frac{1}{4}$ of inch apart. The current was almost entirely stopped by this added decomposition in so bad a conductor; a bubble came from the Volta electrometer only now and then.

2049. The object was to compare the trough with my 20 pr. in the same way, but the obstruction was so great here, it could scarcely have been greater with mine; and there was not enough effect to make it worth carrying on the experiment.

2050. The Large Volta electrometer has been used constantly in these experiments from the time it was made. At first, and for some time, the gas from it had the peculiar electricity smell I have so often noticed from gas in sulphuric acid; but at the xxv expt., and afterwards to the end, the gas had *no smell* of this kind. Hence I conclude it is due to something in the S.A. which has now been exhausted. Probably N.A.

2051. As the smell is in the oxygen always, shall now be able to make out its history. Must add a little N.A. to S.A. and see if it is renewed, etc. Is probably due to a per nitric compound, very volatile, etc. etc.

2052. As good zinc is probably selected for rolling, rolled zinc is likely to be of better quality for troughs than cast zinc.

2053. Different sized plates in one series bad. Holes perforated in them therefore bad, if large; as by corrosion.

2054. xxx. 20 *pr.* 4 *inch porcellain*, which were used on Friday 3 or 4 times and have rested since then. Were cleaned on friday and are in good condition; free from surface copper, thick in metal zinc.

Charge, W. 200; S.A. 4·5; N.A. 4. Action continued 30′.

Gas in Large V. E. = 32 c: i: = 4·0576 gr. water decomposed.

Whole charge 500 c: i:; of these, 10 c: i: gave 65·5 gr. of oxide. The 500 contained therefore 3275 gr. of oxide or 2628 gr. of metal zinc. Each plate had lost therefore 131·4 gr. zinc.

As 4·0576 W. is equivalent to 14·652 Z., so each plate has lost 9 equivalents nearly, or the 20 pr. had lost 180 equivalents of zinc.

2055. xxxi. *Now my* 20 *pr.* 4 *inch*, *common papers*. Have also rested since friday—have not been cleaned—in round trough.

Charge, W. 200; S.A. 4·5; N.A. 4. Action continued 30′—better than xxx.

Gas in L. V. E. = 62·8 c: i: = 7·963 gr. water.

Whole charge 300 c: i:; of these, 10 c: i: gave 78 gr. of oxide. The whole 300 c: i: contained therefore 2340 gr. of oxide or 1877·77 gr. zinc. Each plate had lost 93·89 gr. zinc.

As 7·963 water is equivalent to 28·755 Z., so each plate has lost 3·265 equivalents, or the 20 plates 65·3 equivalents of zinc.

2056. xxxii. Now dismounted *my* 20 *and replaced them* using *recently waxed single papers*, large, so that, as they folded over each other, each pr. of plates was as *well insulated* as by porcellain or glass almost.

Charge, W. 200; S.A. 4·5; N.A. 4. Action for 47′.

Gas in L. V. E. 68·2 c: i: = 8·65 gr. water decomposed.

Whole charge 300 c: i:; of these, 10 c: i: gave 85 gr. of oxide; the whole 300 contained therefore 2550 gr. of oxide or 2046·3 gr. of zinc. Each plate had lost therefore 102·31 gr. of zinc.

As 8·65 W. is equivalent to 31·236 Z., so each plate had lost 3·275 equivalents, or the 20 plates 65·5 equivalents.

2057. This morning, on examining gas evolved from large V. Electrometer, it had *no smell* as in xxx. Nor did I perceive it in expt. xxxi. But the gas from exp. xxxii *had again the smell of Electricity*. On examining the circumstances, I found that in exp. xxxii, I had changed the electrodes so as to render that positive which had been all along negative.

2058. Now it is the oxygen which smells, not the hydrogen (at least I think I noted that). It would therefore appear that there is something in the S. A. which can give the oxygen evolved at the Pos. Electrode the peculiar smell, and which can be separated and reduced at the N. Electrode. Query, Selenium, Tellurium or any of these things. It does not now look like a nitrogen compound.

2059. Rest seems to do something, with porcellain battery at least. See xxx and xxv, also xxii.

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2060. Have to-day experimented on the troughs to obtain comparisons of various acids, etc. and observe the effect of rest, if any. Used my troughs principally. The 40 pr. 3 inch plates were good to the end, so that the comparisons were fair. The 20 pr. 4 inches plates were taken to pieces and repacked with single papers, unwaxed, and were good.

2061. The muriatic acid used contained some sulphuric. 1 c: i: of this M. A. dissolved 108 grains of Marble. 1 c: i: of the N. A. used dissolved 150 grains of the Marble. 1 c: i: of the Sul. Acid used = to 486·1 grains of marble, by an expt. on Carb. Soda crystallized, etc.

2062. xxxiii. 20 *pr.* 4 *inch Porcellains*, same as had been used for 2054. Have rested since then, i.e. since 6th instant. In good condition, but am not sure copper had been cleaned off.

Charge, W. 200; S. A. $4\frac{1}{2}$; N. A. 4. Action continued 40′.

Gas in large V. E. = 37·7 c: i: = 4·78 gr. water decomposed.

Whole charge 500 c: i:; of these, 10 c: i: gave 63 grain oxide. The whole 500 c: i: contained therefore 3150 gr. of oxide or 2527·5 gr. of metal zinc. Each plate had lost therefore 126·37 gr. zinc.

As 4·78 of water are equivalent to 17·26 of zinc, so each plate has lost 7·315 equivalents, or the whole 20 pr. 146·3 equivalents.

2063. xxxiv. *My* 40 *pr.* 3 *inch*, *common papers*; have rested from the 2nd Octr., 2028. Have had no cleaning of the zinc, etc.

Charge, W. 200; S. A. $4\frac{1}{2}$; N. A. 4. Action continued 20′.

Gas in large V. E. = 23 c: i: = 2·916 gr. water decomposed.

Whole charge 150 c: i: Of these, 10 c: i: gave 94 grains

oxide. The whole 150 c: i: contained therefore 1410 gr. of oxide or 1149 gr. metal zinc. Each plate had lost therefore 28·74 gr. zinc.

As 2·916 of water are equivalent to 10·53 of zinc, so each plate had lost 2·7293 equivalents, or the whole 40 pr. 109·17 equivalents.

2064. xxxv. *My* 40 *pr.* 3 *inch*, *common papers*, *with Mur. acid.*

Charge, W. 140; M.A. 11·11. This is equivalent in strength to W. 200; Nitric A. 12. Action continued 20′. Much hydrogen evolved in the trough and could use only 130 c: i: as charge.

Gas in Large V. E., 9 c: i: only ≐ 1·1412 gr. water decomposed.

Whole charge 130 c: i: Of these, 10 c: i: gave 60 gr. oxide. The whole 130 c: i: contained therefore 780 gr. of oxide or 626 gr. of metal zinc. Each plate had lost therefore 15·648 gr. zinc.

As 1·1412 of water are equivalent to 4·121 nearly of zinc, so each plate has lost 3·797 equivalents, or the whole 40 pr. 151·88 equivalents of zinc.

2065. xxxvi. *My* 40 *pr.* 3 *inch plates*, *common papers*, with Sul. Acid only.

Charge, W. 200; S.A. 9. Action about 20′. Much gas in the trough.

Gas in large V. E., 18·5 c: i: = 2·346 gr. water decomposed.

Whole charge, 130 c: i:; of these, 10 c: i: gave 160 gr. oxide. The whole 130 c: i: contained therefore 2080 gr. of oxide or 1669·2 gr. metal zinc. Each plate had lost therefore 41·73 gr. of zinc.

As 2·346 of water are equivalent to 8·471 zinc, so each plate has lost 4·926 equivalents, or the whole 40 pr. 197 equivalents of metal.

2066. xxxvii. *My* 40 *pr.* 3 *inch plates*, *common papers*, with N.M. Acid.

Charge, W. 140; N.A. 4; M.A. 11·11 (= to W. 200; N.A. 18). Action continued 20′. No gas evolved in trough.

Gas in large V. E., 20·5 c: i: = 2·6 gr. water decomposed.

Whole charge, 150 c: i: Of these, 10 c: i: gave 66 gr. oxide. The whole 150 c: i: contained therefore 990 gr. of oxide or 794·45 gr. of metal zinc. Each plate had lost therefore 19·86 gr. of zinc.

As 2·6 of water are equivalent to 9·3886 zinc, so each plate

has lost 2·1153 equivalents, or the whole 40 pr. 84·6 equivalents of zinc.

2067. xxxviii. *My* 40 *pr.* 3 *inch plates, common papers, with common charge*; after preceeding actions and to compare with xxxiv.

Charge, W. 200; S.A. 4½; N.A. 4. Action about 20′.

Gas in V. E. 25·7 c: i: = 3·26 gr. of water decomposed.

Whole charge 140 c: i:; 10 c: i: gave 89 gr. oxide. The whole 140 c: i: contained therefore 1246 gr. of oxide or 1000 nearly of zinc. Each plate had lost therefore 25 gr. of zinc.

As 3·26 of water are equivalent to 11·772 zinc, so each plate has lost 2·123 equivalents or the whole 40 pr. 84·9 equivalents of zinc.

The reason why xxxiv has lost so much more metal is probably that the plates, by exposure to air and moisture for so many days, had become oxidized; and this oxide would produce no electricity though it would appear in the acid.

2068. xxxix. *My* 20 *pr.* 4 *inch, common papers*; have rested from the 6th instant (see 2056). Common charge.

Charge, W. 200; S.A. 4½; N.A. 4. Action for 30′.

Gas in large V. E. 49·3 c: i: = to 6·25 grains of water decomposed.

Whole charge 315 c: i:; 10 c: i: gave 74·5 gr. oxide. The whole charge of 315 c: i: contained therefore 2347 gr. of oxide or 1883·4 gr. of metal zinc. Each plate had lost therefore 94·17 gr. of metal.

As 6·25 of water are equivalent to 22·5687 zinc, so each plate has lost 4·172 equivalents, or the whole 20 pr. 83·44 equivalents of zinc.

2069. xl. *My* 20 *pr.* 4 *inch, common papers*; with S.A. alone.

Charge, W. 200; S.A. 4½. Action for 20′ only. Much gas at trough plates.

Gas in V. E. 7·4 c: i: only = 0·9383 gr. water decomposed.

Whole charge 300 c: i: Of these, 10 c: i: gave 36·2 gr. oxide. The whole 300 c: i: contained therefore 1086 gr. of oxide or 871·5 gr. of metal zinc. Each plate had lost therefore 43·57 gr. of metal.

As 0·9383 of water are equivalent to 3·3882 of zinc, so each plate has lost 12·86 equivalents nearly, or the whole 20 pair of plates 257·2 equivalents.

2070. xli. *My 20 pr. 4 inch, common papers, with M. A. alone.*

Charge, W. 300; M. A. 25 (equivalent to 200 W.; 12 N. A.). Action on for 30′. Much gas in trough.

Gas in large V. E. 11·8 c: i: = 1·496 gr. of water decomposed.

Whole charge 300 c: i: Of these, 10 c: i: gave 37 gr. oxide, so that the whole 300 c: i: contained 1110 gr. of oxide or 890·75 gr. metal zinc. Each plate had lost therefore 49·54 gr. of metal.

As 1·496 of water are equivalent to 5·4 of zinc, so each plate has lost 9·174 equivalents, or the whole 20 plates 183·48 equivalents of zinc.

2071. xlii. *My 20 pr. 4 inch plates, common paper, with N. M. Acid.*

Charge, 300 W.; 25 M. A.; 6 N. A. (equivalent to W. 200 and N. A. 16). Action continued for 30′. No gas from trough. Certain black flocculi, which were swept off from the plates at the first stirring, were redissolved by lying in the acid.

Gas in V. E. 20·3 c: i: = 2·574 gr. water decomposed.

Whole charge 310 c: i: Of these, 10 c: i: gave 52·6 gr. oxide, so that the whole 310 c: i: contained 1630 gr. of oxide or 1308 of metal Zinc. Each plate had lost therefore 65·4 gr. of zinc.

As 2·574 of water are equivalent to 9·295 of zinc; so each plate has lost 7·036 equivalents, or the whole 20 plates have lost 140·72 equivalents.

2072. As to the acids used, 1 cubic inch of the Mur. acid dissolved 108 grains of Marble; 1 c. inch of the Nitric acid dissolved 150 gr. of marble and 1 c. inch of the Sulphuric was equivalent to 486 grains of marble. Hence the relative acid strengths are: *S. A.*, 486; *N. A.*, 150; *M. A.*, 108.

1955	40 pr. 3 inch	Mine	Used	U. W.	W.	S.A.	N.A.	Small V. E.	2	80	
1965				W.					1·65	66	
1996					200	9	4	Large V. E.	2·786	111·5	
1997					200	9	8		2·26	90·4	
1998				U. W.	200	9	0		4·392	175·7	
2001					200	0	8		1·854	74·16	
2003					200	0	16		1·82	72·8	
2004					200	0	32		2·1	84	
2005					200	$4\frac{1}{2}$	4		2·16	86·4	
2006					200	$4\frac{1}{2}$	4	Small V. E.	2·17	86·8	
2007					200	$4\frac{1}{2}$	4	Both V. E.	1·207	48·28	
2028					200	$4\frac{1}{2}$	4	Large V. E.	2·527	101	
2063					200	$4\frac{1}{2}$	4		2·73	109·17	
2064					200	M. A. 15·87	0		3·797	151·88	Charge is equivt. to W. 200; N. A. 12
2065					200	S. A. 9	0		4·926	197	
2066					200	M. A. 15·87	6		2·1153	84·6	Charge equivt. to W. 200; N. A. 18
2067					200	S. A. $4\frac{1}{2}$	4		2·123	84·9	
2008	20 pr. 3 inch			U. W.	200	$4\frac{1}{2}$	4	Large V. E.	2·53	50·6	
2009	10 pr. 3 inch				200	$4\frac{1}{2}$	4		3·725	37·25	
2017	20 pr. 4 inch	Mine	New	W.	200	$4\frac{1}{2}$	4	Large V. E.	2·32	46·4	
2019			Used	W.	200	$4\frac{1}{2}$	4	Both V. E.	4·85	97	
2020				W.	200	$4\frac{1}{2}$	4	Large V. E.	4·47	89·4	
2029				U. W.	200	$4\frac{1}{2}$	4		3·864	77·28	

2047				U. W.	200	$4\frac{1}{2}$	4	Large V.E.	3·57	71·4	
2055					200	$4\frac{1}{2}$	4		3·26	65·3	
2056				W.	200	$4\frac{1}{2}$	4		3·275	65·5	
2068				U. W.	200	$4\frac{1}{2}$	4		4·172	83·44	
2069					200	$4\frac{1}{2}$	0		12·86	257·2	
2070					200	M.A. 16·7	0		9·174	183·48	Charge equivt. to W. 200 + N.A. 12
2071					200	16·7	4		7·036	140·72	Charge equivt. to 200 + N.A. 16
2018	10 pr. 4 inch			U. W.	200	$4\frac{1}{2}$	4		6·758	67·58	
2013	40 pr. 4 inch	Porcellain	Used		200	$4\frac{1}{2}$	4		3·544	141·76	
2044					200	$2\frac{1}{4}$	2		4·7	188	
1968	20 pr. 4 inch							Small V.E.	5·36	107·2	
2015					200	$4\frac{1}{2}$	4	Large V.E.	5·25	105	
2027			New		200	$4\frac{1}{2}$	4		3·7	74	
2039			Used		200	$4\frac{1}{2}$	4		5·9	118	
2043					200	$4\frac{1}{2}$	4		14·43	288·6	
2054					200	$4\frac{1}{2}$	4		9	180	
2062					200	$4\frac{1}{2}$	4		7·315	146·3	
1970	10 pr. 4 inch							Small V.E.	16·9	169	
2016					200	$4\frac{1}{2}$	4	Large V.E.	14	140	
2040					200	9	8		24·435	244·35	
2045	20 pr. 4 × 8 inch				200	$2\frac{1}{4}$	2		6·54	130·8	
2041	10 pr. 4 × 8 inch				200	$4\frac{1}{2}$	4		25·68	256·8	
2046	10 pr. 8 × 8 inch				200	$2\frac{1}{4}$	2		16·4	164	

2073. Expts. on Mr William Jenkin's Magneto-electric shock from a single pair of plates.

2074. Used his helix and iron rod but my circular Electromotor. His helix about 2 feet long and the rod the same length and about half an inch in diameter. My electromotor a single pair exposing about 2 square feet of surface of zinc.

2075. The spark on breaking contact (when iron in the helix) much more powerful than on making contact; the latter happens very rarely. The spark was decidedly brighter when the iron was in the helix than when out.

2076. If a bolt of copper in the helix instead of the iron, then the spark not improved.

2077. The shock obtained by grasping two cylinders soldered on to the ends of the helix wires also very good on breaking contact if the iron were in; but none if the Iron were out.

2078. Now used a small Voltaic pair of plates and introduced both the helix and a galvanometer in the circuit. When galvanometer steady, removed the iron from the helix, but there was no change in the deflection. Hence the current constant, whether the iron in or out, and the quantity in a given time constant. Notwithstanding that, there must be a change in the current or in its *character*, or else a brighter spark could not pass on breaking contact. Whether Iron in or out current is in the *same direction* and of *same strength*.

2079. The spark is from a current passing one way through the wire of the helix. The shock is from a current passing the other way, and is a pure Magneto electric shock consequent upon the passing off of the Magnetic state of the iron so soon as the principal current ceases.

2080. Thinking the difference of spark even without the iron striking from a short wire, tried results with a long and short wire.

2081. A short connecting wire gave but a small spark on breaking contact; the whole circuit of the helix a greater, and the helix with iron in a still greater. Hence effect of elongating the wire and putting soft iron in the same, and is like a kind of draught

in the metal, making the discharge at the moment of breaking contact able to pass over a larger space of air, etc. Being in fact at that moment more intense.

2082. A wire (copper) of $\frac{1}{18}$ inch in diameter, of 114 feet long, was used to discharge the Electricity of the large Electromotor. When by crossing the wire the circuit was through 12 inches only the spark on breaking was small, although the wire conveyed so much electricity as to become quite hot. When the discharge was through the 114 feet the spark was much brighter, although the wire remained cold. Here also the larger spark was produced by the smaller current of electricity.

2083. A copper wire $\frac{1}{9}$ of an inch in diameter was used; 12 inches gave a small spark; 36 feet a much larger. Even larger than the former wire of 114 feet.

2084. Hence use long length of thick metal rod or pipe and try again.

2085. Used our large soft iron ring electro-magnet. Here the spark upon breaking contact excellent, and the shock also.

2086. Upon leaving the poles open or closing them with the keeper there was no sensible difference.

2087. Tried to make out the direction of the Shock current experimentally, but did not succeed. Must take the helix farther away from the galvanometer used.

2088. The spark in these cases can be no proof of the quantity of the current previously passing. Nor probably of its intensity. But at the *moment* of passing it must indicate a higher intensity and a larger quantity.

2089. There is evidently an affection of the current independant of quantity passed. If it be independant of intensity also, then what quality of the current is it which is thus rendered evident by its exaltation?

2090. It is I think evidently dependant upon induction. See the paper I have written to the Phil. Mag. Though the experiments were made on the day of the date given, the rough notes were not entered here until the 20th Octr. at Brighton.

2091. Fluorine evolved from Fluoride of Silver by Chlorine acted on Caoutchouc strongly. Aimé, Ann. de Chimie, lv, 443.

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2092. Electrotonic wire, i.e. effect of long wire, magnet, etc. etc. Are effects of induction of *currents* over *currents*. May well be called the *electronic* state of a current. But the forces are well represented and related by the magnetic curves.
2093. The effect may be said to be as follows. Suppose a current (constant) to be subject to the induction of another current. As the second current is *formed* in the vicinity of the first, if in the same direction, it for the moment diminishes the first current; but that first current soon returns to its first amount and then has the peculiar state. When the inducing current is removed the peculiar state falls, and the induced current is for the moment increased. It then sinks to its natural quantity, and if the inducing current be renewed in the reverse direction to what it had at first, the induced current will increase for a moment; then (probably) have a state the opposite to that I describe, which it will keep until the inducing current ceases, when it will be found to diminish in quantity for a moment, and then rise to its full standard, free from all extraneous influence.
2094. As one wire or current act on its neighbour wire or current, there can be no doubt that the current in one part of a wire acts on the current in another part parallel to it; or that every part of a current acts on the other parts of the same current lateral to it. Hence peculiar mutual action and relation of the elements of an electric current which are in the same plane across the course of the current. Hence use of increasing the wire.
2095. Ascertain that when action opposed also, the quantity passed is the same. Has been done in first series of Researches.
2096. The action on the subject currents—and all are reciprocally subject—is the endeavouring their continuance if all are in the same direction, or endeavouring their sudden cessation if the currents are contrary to each other. But the effects are not effects of alteration in quantity. Must be of the intensity at least.
2097. Ascertain really if intensity the same.

2098. Will a current, not able to decompose iodine, decompose it when helped by a stationary magnet?
2099. Can thermo-electric currents be helped by it?
2100. Must make a separation in the quality of intensity and perhaps find its basis here.
2101. Common electricity has same action. Try it with a magnet in helix, by P. and N. Sparks, etc.
2102. The effect of kite string, or Ronald's or Wheatstone's wires, a probable consequence of this.
2103. If electricity were going through a given quantity of wire in one large or 100 small wires (the latter apart), there should be difference in the results produced; because in one mutual induction, in the many that avoided. How will that affect the subject of conduction; or how is the difference rendered evident in these two sets of wires, i.e. by what difference of effect?
2104. The mutual inductive action of currents must have existence to an enormous extent within a magnet; and probably produces some very important consequences there. As it helps to sustain the existence of the currents it may have to do with the permanent magnetism of iron and steel. Still, at present it always seems the consequence of currents previously formed.
2105. Is it possible that, a current being produced, its inductive effect should in some way, or in some cases, as in iron, even surpass in its tendency to sustain or produce a current, the first force; and then would not the currents continue, and so a permanent magnet result? See, in way of analogy, Airy's paper on possibility of the conditions for a perpetual motion. *Cam. Transactions.*
2106. Ampère's elongating wire probably only a fact associated with or belonging to the Electro tonic state of current; and perhaps also my rising wire.

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2107. Have constructed a bundle of coils or helices consisting of four helices, one over the other, upon a pastboard cylinder, so that a rod of Iron or steel might be put within. All the helices were in the same direction, and were separated from each other

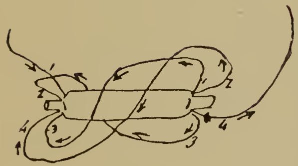
by calico, and the convolutions of each helix from its neighbouring convolutions by an interposed string.

The first helix, or that next the pastboard, contained	48 feet of wire (copper);
the second helix, or one above it, contd.	$49\frac{1}{2}$ feet „ „
the third helix	48 feet „ „
the fourth or outer helix	45 feet „ „

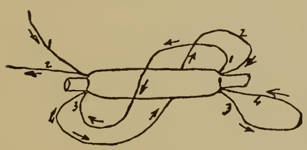
The wire was all copper and of the same size, i.e. about the of an inch thick. The wire at the ends of these helices were left projecting for about a foot each beyond the lengths given, so as to allow of communication in various ways. It was found, upon trial by a voltaic circle, that none of these helices communicated through the calico or string with the others, but that each acted distinctly and separately when called into use.

2108. The helices 1 and 3 were connected together, being soldered at *a* so as to form one circuit which, though consisting of two helices, were from their symetrical arrangement virtually but one helix, and this consisted of 96 feet. Then a length of 97 feet of the same sized copper wire was laid down upon the floor of the laboratory, and the ends of these two equal circuits were amalgamated and brought up to an electromotor consisting of a single pair of plates, to observe the relative intensity of the spark produced upon breaking contact. By alternating the experiments it was easy to see which gave the brighter spark. In this way it was found that the spark from the wire in the form of a helix was far brighter than that from the straight or extended wire; and thus the effect of the mutual induction of the different convolutions of the helix fully shewn.

2109. A short wire about 9 inches long, being used at the same time with the same electromotor, gave scarcely a sensible spark.



2110. Then all four of the helices were arranged as one consistent helix, so that the current should be in the same direction in all the circumvolutions. This gave a good spark on breaking contact.



2111. But when the whole of the helices were made into one circuit, but so that two of them should be opposed to the other two in their direction, then the wire gave a spark scarcely perceptible upon breaking contact, and not brighter than that of the short length of 6 inches.

2112. Thus the whole effect of the length of wire was neutralized by the reciprocal and contrary action of the two halves which constituted the helices in contrary directions.

2113. By another experiment I found that, when these contrary helices were removed from each others influence, then there was no longer an injurious effect. The spark passed well through the whole length of wire, and better than in the straight wire, because the different circumvolutions of each half could act on and assist each other, and because at the same time the equal and opposing actions of the two halves were not allowed to come into play.

2114. The helices 1 and 3 were joined together so as to make one conformable helix of 96 feet of wire. The helices 2 and 4 were left out of use, the ends being in the air and unconnected. A circular magnet was put into the cylinder and the spark upon breaking contact then observed.

2115. When the magnet was in, it helped a little, and that whether its position was such as to *accord* with the magnetism of the helix or be *opposed* to it. I did not see any difference in that respect. At the same time, it did not help the effect nearly so much as an equal piece of soft iron.

2116. A piece of hard steel did not produce nearly so good an effect as a piece of soft iron.

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2117. Twenty-eight feet of copper wire were made into one regular helix upon a cylindrical magnet, only two thicknesses of paper intervening. When this helix was used to connect the plates of the Electromotor, the spark upon breaking contact was of the *same brilliancy and size* (sensible), whether the electric current was passed through the helix in accordance with the magnetism of the magnet, or in the opposite direction. Hence the magnet has no influence as a magnet over the phenomenon.

2118. When the current accorded with the magnet the power of the latter was *increased*, but as soon as the current was stopped the magnet fell in force. When the current was opposed to the magnet the power of the latter diminished and was at times even reversed, but when the current stopped, the magnet returned to its first state and former polarity, if not in extent of power, at

least in direction. Hence the magnet acts at these moments like a piece of soft iron, but because of its hardened condition it cannot change so far under the influence of the current or afterwards drop through as great an extent.

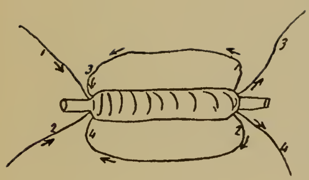
Magnet in this respect just as unmagnetised steel.

2119. Now loosened the magnet and took it out of the helix, and then used the helix (of 28 feet), sometimes alone, sometimes with the magnet and sometimes with an iron core. The helix alone gave a good spark; with the magnet perhaps a little better, and if better, equally so whichever way the magnet in; with the iron core much better. If the magnet acts at all, it acts merely as iron—perhaps equal to a moderate Iron wire.

2120. Now experimented upon this helix with the view of comparing the same wire in different forms and arrangements. As a helix (no iron or magnet being in it), it gave a good spark upon breaking contact. Then being suddenly pulled out into its full length, so that the circumvolutions could no longer act by mutual induction, it gave a much smaller spark; and finally being made up into a helix, one half in one direction and the other half over it in the reverse direction, it gave a spark so small as to be seen with difficulty and just like a short piece of wire.

2121. A piece of thick lead pipe, 6 feet lông, was brought into the discharging circuit by means of two pieces of wire each 3 feet long. The 12 feet did not act better than 12 feet of straight wire. The massiveness of the lead pipe did not seem to do anything.

2122. A piece of the wire from 8 inches to 2 feet gave scarcely a sensible spark.



2123. The compound cylinder (2107) was arranged as two separate conductors, the one consisting of helices 1 and 3 connected consistantly or symetrically, and the other of 2 and 4 also connected in the same way. Hence it was easy to send electric current through both these conductors from two separate electromotors, and make them accord or oppose at pleasure. The electromotors were each a single pair of plates (cylinders) and nearly equal in power.

2124. Being thus arranged, I found that either current in either helix could give a good spark on breaking contact, but that when a current was passing through one helix at the time the current in the other was broken, scarcely a sensible spark passed.

2125. It did not matter whether the currents through the two helices were in the same or in contrary directions, or whether the one was stronger or weaker than the other, or at which helix the disjunction was made; the effect was always the same and the spark nearly annihilated.

2126. When disjunction of both helices were made in succession, the first which was separated gave a very weak spark; the second a strong one.

2127. Here the permanent current seems to have an influence over the whole phenomenon, but its interference in the same way, whether in the same or contrary direction, is very anomalous. I found, by an after experiment to be described, that in fact the current had no influence whatever and that the effect is due to another circumstance.

2128. All the effects seem to point out that the peculiar action occurs only at the moment of breaking the contact and stopping the current; and as it is evident from the action of a helix that the neighbouring convolutions do affect each other and increase the effect, I thought it must follow that a *neighbouring* current from a separate Electromotor must also have the same power; but probably limited to the moment at which it is suddenly stopped.

2129. I arranged the apparatus therefore so that, by means of two amalgamated plates, upon which rested the ends of two amalgamated wires, both attached to a cork and properly connected with the two interposed helices and the two electromotors, I could break the contact of both at the same moment.

2130. In this way I expected that, if the currents were in the same direction, both wires would have their sparks increased at the moment of breaking contact; that if they were in contrary directions and equal in power, the sparks would be diminished almost to nothing.

2131. But I found it almost impossible for me to obtain both sparks accurately at the same time by my apparatus; one always preceeded the other. Now, simultaneous existence is *essential* to the experiment. This perhaps might be done by a very perfect apparatus, but the velocity of Electricity is such that it will be difficult to do.

2132. The apparatus shewed well the apparent destruction of the

effect at the interrupted circuit by the vicinity of the continuing circuit.

2133. Considered that, in the apparent destruction of the effect in one helix by the continuance of the current in the other (   ), the effect most probably did not depend upon the current, but upon the circumstance that the second helix formed a closed circuit, through which any induced current might circulate. I removed one electromotor, and connected the ends of its helix together, and then found that the spark could no longer be obtained at the other. Hence, when the electromotor was used, it merely served to connect the ends of its helix, and its current went for nothing. Hence cause why direction of current produced no effect.

2134. Now then begin to see light. The phenomenon of increased spark is merely a case of the induction of electric currents. If a current be established in a wire, and another wire forming a complete circuit be placed parallel to it, at the moment the current in the first is stopped, it induces a current in the *same* direction in the second; itself then shewing but a feeble spark. But if the second be away, it induces a current in its own wire in the same direction, producing a strong spark. The strong spark in the current when alone is therefore the equivalent of the current it can produce in a neighbouring wire when in company.

2135. All the phenomena of the magnet and soft iron agree.

2136. These effects shew that every part of an electric current is acting by induction on the neighbouring parts of the same current, even in the *same wire* and the *same part* of the wire.

2137. There must be a reverse effect of this kind, when contact is made, which will tend to diminish the spark which can then pass.

2138. Similar effects must occur with a battery. Hence great difference between spark on making and on breaking contact.

2139. Must try to obtain some effects on making contact.

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2140*. Made the two helices of (2107) into one and used one electromotor; found the spark greater than if only one helix used. The two helices were joined, not end to end, but as in the figure, so that the current, entering and passing out by single wires as

* [2140]

at *a* and *b*, divided in the course of its passage into two equal currents having similar directions. On breaking the contact at *a* the disjunction was of course simultaneous for both currents; an effect I could not obtain mechanically. If these two helices had not had power to act on each other, i.e. had been at a distance from each other, the spark would have been less than that actually obtained, and which, on being compared with that obtained on using one of the helices only for the whole current, was better than the spark so given. However, it is only by separating the helices one from another that that can fairly be established.

2141*. Now crossed the helices so that the current, when divided, went in contrary directions through the two. In this case, on breaking contact at *a*, there was no sensible spark scarcely; only the effect of a short wire. Yet the current went through both, but now at moment of disjunction neutralized each other. Good; shews effect of simultaneous disjunction.

2142. These phenomena must happen in cases of common electrical discharge through long wires, but then simultaneously. Hence some curious results; but what are they?

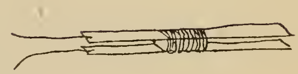
2143. Must use the soft iron Electro magnet arrangement in discharge of a voltaic battery. Will it affect the spark either when latter continuously or at termination of discharge?

2144. Have made some mistakes in letter to Phil. Mag. last month as to direction, etc.; correct them next month.

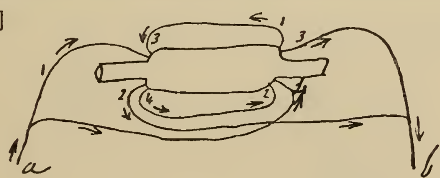
2145. Laid down on floor of laboratory 132 feet in length of thick copper wire, 0·18 of inch in diameter, with copper handles at two extremities so as to give extensive contact with hands moistened in brine. With this, obtained a very good spark on breaking contact, shewing the effect of long straight wire; but could not obtain the shock which I expected.

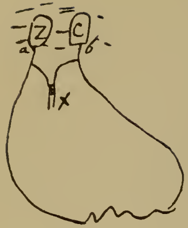
2146. The soft iron Electro-magnet arrangement gave a very good shock at disjunction when used with the same *electromotor*— but then it gave a much larger spark.

2147. At last succeeded in obtaining the shock from the straight wire, thus. Two plates of silver were fastened to a cork so that the tongue could be introduced between the space at one end of the arrangement, and wires proceeding from the other ends of the plates were attached to the long wire near the extremities, as



* [2141]





thus. Then the tongue, being placed between these ends, as at *x*, and the contact of the long wire and the electromotor broken either at *a* or *b*, a strong shock was felt in the tongue or lips at every disjunction.

2148. No taste or sensation was felt on making junction, nor even when the plate arrangement was separated from the long wire and used directly with the electromotor. The current from the latter could not in this case complete the circulation through the tongue so as to produce taste.

2149. The tongue arrangement used with the large soft iron electro-magnet gave very powerful shock in the mouth, etc.

2150. When the shock on hands or tongue was obtained from either the straight wire or the Electro magnet coil, still there was no sensible diminution at the place of disjunction of the brilliancy of the spark. Hence the electricity as shock is only a small portion of that which passes as the result of the inductive action.

2151. If a good conductor, as a wire, be used instead of any part of the human body, then there is scarcely a sensible spark at the place of disjunction; the induced wave of electricity then going through that wire. It is thus easy to separate the induced portion from that which passes only by virtue of the electromotor, and which is the same whether a long or a short wire be used.

2152. Now looked for this induced wave of electricity in the form of a spark, not at the place of disjunction with the electromotor, but at the place where the shock has been felt, using subsidiary cross wires there; and obtained it both with the soft iron electro-magnet and also the long extended wire arrangement.

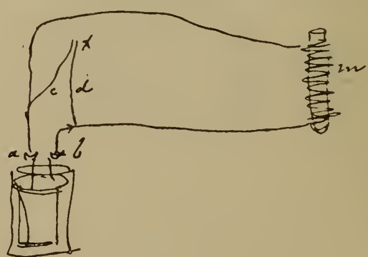
2153*. Let *m* be the electromagnet, the wires of which are communicated with the electromotor at *a* and *b*; let *c* and *d* be the subsidiary wires.

2154. When *c* and *d* were in contact at *x*, whilst the junction was broken at either *a* or *b*, then no spark or scarcely a spark visible there.

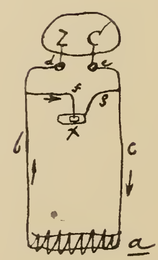
2155. When junction continued at *a* and *b*, and contact at *x* broken, a small spark visible there.

2156. When contact broken at *a* or *b* whilst the ends of *c* and *d* were brought so near together at *x* as to allow a spark to pass, then a bright spark did pass there, and no spark at *a* or *b*.

* [2153]

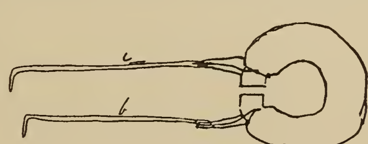


2157. When contact at *x* broken first, then the usual bright spark at *a* or *b* on disjunction there.
2158. The same things happened when the long wire ( ) was used instead of the electromagnet, but the sparks were not so bright.
2159. The spark at *x* I consider as the one due to the induction at falling or ceasing of the current, and to be equivalent with, or in fact the same as, that usually shewn when the subsidiary wires are not used upon causing disjunction at *a* or *b*.
2160. I suppose that the spark which occurs at *x* upon disjunction there whilst the contact at *a* and *b* is continued, is in one direction, and the brighter spark which occurs there when contact is broken at *a* or *b* to be in the other direction. But examine these more closely. Some good results may be obtained here as to current induced on making as well as breaking contact.
2161*. Then arranged the soft iron Electro magnet so as to obtain evidence of the course of the induced current by chemical indications. This Electro magnet consists of a cylindrical bar of soft iron inches long and inches in diameter, bent into a ring and surrounded by three coils of thick wire, the ends of which were made fast to two thick rod or wire conductors. Hence the electric current in it was divided into three streams in the helices, all according in direction.
2162. This was arranged with the Electromotor thus: Z and C represent the plates of the electromotor; *a* the electromagnet; *b* and *c* its terminations, making contact with the electromotor at *d* and *e*; *f* and *g* are the two supplementary or subordinate wires where the chemical action of the induced current was to be examined; *f* terminated in a platina wire resting on paper moistened in hydriodate of potassa, which was supported on a platina plate attached to *g*.

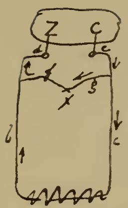


2163. In this form of experiment, when contact was made and continued, the current moved in the principal circuit as shewn by the arrows; *no* current passed through *f* and *g*, for no decomposition took place there, the principal wire being able to carry all the current. But on breaking the contact at *d* or *e*, then iodine evolved at *x* against the end of the wire *f*. This was from action of the induced current, and shews that direction from *f* through

* [2161]

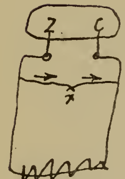


the paper to *g*. A spark also passed at the same time at the place of disjunction, as happened in the case of the shock.



2164. But when *f* and *g* touched metallically, there ought to be a current through them in the opposite direction whilst contact continued, because they then carry part of the current urged onwards by the electromotor; *b* and *c* carry the rest of that current. This was proved by putting a galvanometer at *x*, for whilst the contact was continued there was a permanent deflection, shewing a current as marked by the arrows.

2165. On breaking the contact at *d* or *e* the needle (previously allowed to obtain its permanent state of deflection under the influence of the current), swung back and passed the magnetic meridian or neutral line by a greater number of degrees on the opposite side to those measuring its permanent deflection; as if, when relieved from the permanent current, it had not merely been urged back by the earth's force only, but that some additional force from a counter current was produced. And this was shewn beautifully by putting stops in the way of the needle on one side, so as to prevent its leaving the magnetic meridian whilst the principal current was continued, for then, on breaking the contact, the induced current acted, and by swinging the needle far round in the opposite direction, shewed that it was as marked in the margin; being like that indicated by the chemical action and contrary to that occurring during the continuance of the contact.



2166. On using the long wire only I could obtain no signs of chemical decomposition.

2167. Using the galvanometer at *x* with the long wire (   ), there was a strong direct current whilst contact continued and but a very feeble reverse current when the contact broken with the Electromotor.

2168. On making the experiment for chemical effect with the soft iron Electro magnet, no chemical action could be obtained, or evolution of iodine against the end of *g* at *x* (2162). This is a good experiment, as shewing the low condition of the current when *contact is made*; for the current which the electromotor tends to urge through the principle conductor is at the first moment diminished or neutralized by the counter current which tends to form.

2169. Important for general principles to shew that all these effects, which a current can produce on itself, can be produced on it also by a neighbouring and independant current.

2170. It seems to me as if there must be some intermediate link of action between the current as produced from the electromotor and that occurring at moment of disjunction; and that this link is also the same with that which connects electrical and magnetic properties.

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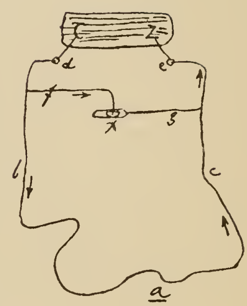
2171. See, in reference to this subject, 92, 181 of written notes[1].

2172. Is Ampère's elongation of the current a connected phenomenon—also my elevations in my early papers, Quar. Jour.?

2173. Repeated the experiments with the wire ( ), using a new strong charge of Nitro sulphuric acid. The arrangement was now as in the figure.



2174. When the contact was continued, obtained iodine at *x* against the end of *f*, shewing a permanent current from *f* to *g*. This is therefore a part of the original current which the long wire *a* under the circumstances was unable to carry.

2175. On breaking contact, I could obtain no chemical effect at *x* indicating a current in the reverse direction; but this could hardly be expected, for as during contact a direct current existed, the effects it could each time produce on the iodide would neutralize those of the reverse current. It was in fact impossible under the circumstances to separate the two sets of effects from each other, and though the reverse current was probably the most intense, the direct current must of necessity have been of the longest duration.

2176. Then again, the inducing force of the simple wire is far less than that of the soft iron electromagnet, and, as appears, its conducting power also; both circumstances being against the developement of the induction results by chemical means.

2177. I observed that if contact were continued at *d* and *e*, the decomposition at *x* soon diminished and at last almost ceased; but that on breaking contact at *d* or *e*, the power of decomposition at *x* was restored. So that continuing contact for 5 or 10 seconds would produce very little effect at *x*, whereas, if contact were

[1] Pp. 16, 32 of this volume.

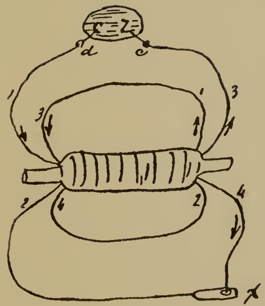
broken 10 or 20 times, during those 5 or 10 second[s] a much stronger effect of decomposition at x would result, but always in the same direction. This effect is probably due to the surface of platina at x assuming Marianini's state, which is let down by the counter current each time the contact at d or e is broken. Perhaps also part of the effect may be due to the direct current through x being stronger when *contact* is made at d or e, as a consequence of the resistance or counter current which tends to form in the principal wire when the contact is established.

2178. Now placed a galvanometer at x instead of the decomposing apparatus. It gave very strong indications of the direct current, or that produced whilst contact continued, shewing that more passed through this subordinate communication, in the present case of the long wire, than when the electro magnet was used. On stopping this deflection mechanically, as before ( ), and then breaking contact, I obtained but very feeble indications of the reverse current. Must repeat the expt.

2179. The spark was however wanting at the place of disjunction and the shock from this long wire on the tongue was always good and constant on breaking contact.

2180. As there must be some action and effect on the making of contact which shall correspond to that which occurs on breaking contact, but as from every principle of action and reaction that must be in the reverse direction and tend as it were to diminish the spark, or at least to prevent its appearance, it becomes very difficult to obtain this negative effect.

2181. May however consider and estimate the effect of *contact at first* by observing what happens in a lateral wire, for we are able to transfer the force from the wire carrying the original current to another wire, and there shew its effects ( ). This has been done in some cases with the double helix, but now rearranged it so as to give evidence of chemical action.



2182. The independance of the two helices was first ascertained. No iron core or magnet was used. Then one helix was connected with the electromotor and had a current sent through it, whilst the other was associated with a decomposing apparatus. Whilst the current through the first helix was continued, no decomposition took place at x; but on breaking the contact at d or e, obtained

a decomposition at x, iodine appearing against the end of the wire 4 and indicating, as it should, a current in the direction marked.

2183. Tried to obtain the reverse current on making contact but did not succeed distinctly by chemical means. My arrangement however very sudden and imperfect.

2184. Put a galvanometer at x, and then easily obtained the two currents, one on making and the other on breaking contact.

2185. I now, without much expectation of results, put a galvanometer into the direct circuit of a current sent through the soft iron Electromagnet. When contact was made it swung at once forcibly in the right direction, giving no indication of a preliminary action. When the needle was mechanically confined in the magnetic meridian, breaking contact cause no reverse deflection except the very little which resulted from the residue of mechanical forces not perfectly balanced by the stops used.

2186. All the effects at making and breaking contact, separated when a current is used by an interval of time, must occur at once in the common electrical spark when taken from a wire. Must occur at once even, to a small extent, when a short wire or a conductor of any length, however small, is used. Must occur therefore in every electrical spark. And further, probably occur at every instant in every part of a continuous electric current, but are only insulated at the extremities; just as where, in a spectrum but little developed, the colours appear only at the extremity—white being in the middle; but not white consisting of the identical association which made white on entering the prism, but by the overlapping of successive coloured spectra.

2187. The effect of wire on wire, of convolution on convolution, and magnet and wire on each other, shews that the force exerted is lateral, as it were, in its direction; according thus with the magnetic curves and being represented in its direction, etc. by them.

2188. That the original current should be able to exalt and sustain a power which can react like a spring when the original current ceases, and with an intensity very much greater than that original force, implies, I think, some intermediate or collateral state of the force or forces aside from direct electrical action—for the

reaction is higher in the quality of intensity than the original action.

2189. I cannot find that the intensity during the continuance of the current is increased, i.e. that the soft iron electromagnet can make a current passing round decompose a substance which it could not decompose before.

2190. Must transform these results into such as can be obtained with magnets only, when wires move across their magnetic curves, etc.

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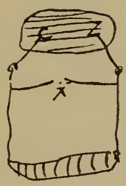


2191. Used the Electro magnet, and a fine wire at x or the place where the cross wires met. The wire was platina. All remained quiet and unchanged whilst contact at C and Z continued; but on breaking contact there the wire at x was *ignited and fuzed.*

2192. Then used a thicker platina wire there, and could so adjust it as to length and thickness that the wire should not be fuzed; but became ignited each time the contact was broken at C and Z, though it remained black and cold whilst contact continued. Other effects at x supervened, as that of first contact, etc. etc., but the effects required were very easily distinguished.

2193. If a thick wire with a cut end be used, with a single pair of plates to complete the communication, and whilst contact is continued either at C or Z, the contact at the other side is made by just approaching the extremity of the wire to the surface of the mercury, bright sparks and combustions of the metal may be obtained; and it looks, according to circumstances, either as a continuous discharge or the spark of making contact. In both cases, however, it is generally the spark of breaking contact. The first contact causes ignition of the points touching; that agitates the mercury, drives it away, and so breaking contact, gives the bright spark; and when the tremors of the mercury repeat this action, it becomes to ordinary observation a continuous spark.

2194. The effect can be produced with a short wire, but infinitely better with a long one or with an Electro magnet arrangement.

2195. So quickly travels the wave of electricity in a wire that the shortest sensible contact is long enough to give the full effect when the contact is broken.

2196. When spark is taken between platina surface, then is purest, i.e. freest from effects of combustion. Is very good on breaking contact when large Electro Mag. used, but scarcely sensible on making it. It is of course nothing like so bright as that obtained when mercury used.

2197. The spark on mercury probably not so instantaneous as that between platina.

2198. Using my battery of 46 pr. 5 inch plates, the effects with the electro Mag. on making and breaking contact were very distinct, and of the same kind as those with a single pair of plates. There was nothing further striking in them.

2199. The *permanent* discharge of a battery was not better or worse, or at all different apparently, whether the large Electro magnet was in the circuit or not.

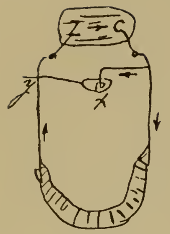
2200. Took a wire 20 feet long, covered with silk, and doubling it in the middle, bound it round with string into a length of double wire of 10 feet; the two halves of the wire being close to and parallel to each other. Using this wire with the single pr. Electromotor, scarcely a sensible spark occurred on breaking contact; but when the binding thread was removed, and the two halves were separated and free from each other's inductive and contrary actions, the same wire gave a good spark on breaking contact.

2201. If of two parallel wires, one were used as a conductor and the other to make a circuit open or complete at x at pleasure, there is no doubt that, whilst the second wire open at x, the first would give a good spark on breaking contact; but that if the second were closed at x, the first would give little or no spark.

2202. Opposed helices conjoined in one circuit produce no effect of spark on disjunction (2111, 2141). Putting an iron core in was expected not to alter the result. Tried this morning and it was so. No change was produced. All null.

2203. Repeated the expt. of decomposition at the cross wires when the Electro magnet was used as the conductor and a single pair Electromotor as the generator of Electricity (2168).

2204. On former occasion, on continuing contact at Z and C, there was no chem. action at x from a portion of the current passing by that channel. See 2162, 2168. On using a stronger charge, with more N. Acid in it, I could obtain a feeble effect



at x whilst contact with Z and C continued, shewing continual passage of a little there. But if I proceeded in the following order I obtained a much better effect at x. Break contact at y, then break contact at C or Z; renew contact at y, then renew contact at C and Z; and so on many times. In this way I avoided any current through x due to breaking of contact at C or Z, and obtained there the effect of *making contact*, and which effect was greater than that due to the constant current. Does not this shew that, at the moment of making contact, there was an effect of retardation in the part of the circuit constituted by the Electro magnet, which threw more into the circuit x; and that, contact being established, this effect of retardation passed away? The Electro mag. part acquired its standard and constant conducting power, and then, as we know, on breaking contact rose in conducting force beyond its constant condition. The retardation at first and the exaltation at last are the effects due to making and breaking contact.

2205. These effects might be well shewn by a galvanometer at x blocked so as to give the excesses of power in the different directions on making and breaking contact.

2206. Tried for similar effects with a long wire, but did not obtain them distinctly. In fact, a wire of equal length and thickness as that round the Electro magnet is so much weaker in the peculiar effect on making and breaking contact, whilst at the same time it is quite as bad a conductor, that one could hardly expect to obtain results which had not come out even with the Electro magnet in the first instance.

2207. Tried again to obtain the reverse current at x by a galvanometer when a long wire formed the circuit (   ); and for that purpose blocked the galvanometer in the natural position on one side, i.e. by blocks at opposite sides at each end. Still I did not succeed. With the electro magnet it is easily obtained, and there are plenty of reasons why here it should be more difficult.

2208. Repeated the expt. 2182, in which a current through one helix was made to induce a current through another helix associated with it, either at the making or breaking of contact. The helices used were the same, but the Electromotor was my battery of 46 5-inch plates, and the effect of making or breaking contact was

repeated several times before the result was examined. Now obtained decomposition of the solution of iodide for the current induced at making and also that induced at breaking of contact.

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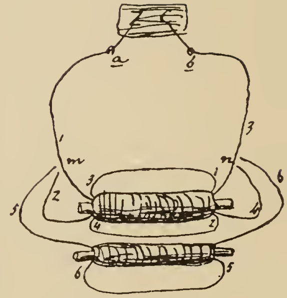
2209. Have made a new helix of two coils, each containing 46 feet of the same wire used in the former helix (2107). The cylinder and convolutions were of the same size, so that this helix, made into one circuit, is equal nearly to half of the former helix.



2210. In this arrangement, the wires 1 and 3 make a helix of 96 feet in convolutions, and its extremities can be connected or disjoined at *a* and *b* with an electromotor. The wires 2 and 4 make a helix of $94\frac{1}{2}$ feet, in the same direction and close inducting association with the first, and which can be made to divide the current with it by bringing 2 and 4 ends into contact at *m* and *n*. The wires 5 and 6 constitute a third helix, containing 92 feet in convolutions, at a distance from the first helix and having no inductive action in common with it; and this may be used instead of the second to divide the current with the first.

2211. When helix i alone was used, there was a certain bright spark on breaking contact.

2212. When helix ii was associated with i at *m* and *n*, there was a brighter spark on breaking contact at *a* or *b*.

2213. But when helix iii was used instead of helix ii, there was a smaller spark—smaller spark even than when i was used alone.

2214. So that helix iii joined to i had not such power as ii joined to i, and mainly because the induction absent.

2215. There are some circumstances to notice here important. Helix iii touching i gave smaller spark than i alone, and at last it caused the disappearance of the spark at *a* or *b* altogether. In this case it evidently acted as a return or cross wire to i, or perhaps i as a return or cross wire to it; for either alone gave a good spark on breaking contact at *a* or *b*.

2216. This effect is possible and perhaps probable unless the two helices are perfectly alike in all things, i.e. thickness of wire, nearness of convolutions in each, contact with each other and the Electromotor, etc. etc.; and in many cases the effect may be, not to destroy the spark entirely, but only diminish its brightness.

2217. These effects will have a connexion with the comparitive conducting power of the whole length of wire acting as the cross wires, and the conducting power of the layer of fluid between the plates of the electromotor, for the electricity will pass one way or the other.

2218. If soft iron were put into either helix the other would most likely act as cross wire to it. Try this.

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2219. xliii. *My* 40 *pr.* 3 *inch plates, old, common papers, no glasses.*

Charge, W. 200; S. A. 4·5; N. A. 4. Action continued 25 minutes. The large volta electrometer has been taken to pieces and newly arranged; the plates were now above half an inch apart.

Gas in large V. E. = 29·3 c: i: = 3·715 gr. of water decomposed.

Whole charge 140 c: i:; 10 c: i: gave 86 gr. oxide zinc, so the 140 c: i: contained 1218 gr. of oxide or 977·45 zinc = 24·436 loss per plate.

As 3·715 W. is equivalent to 13·415 Z. nearly, so each plate has lost 1·82 equivalents, or the whole 40 lost 72·8 equivalents of zinc.

2220. My battery of 56 pr. 5 inch plates. It acted beautifully at first, i.e. when new; but having been used perhaps 8 or 10 times, it has fallen very much in power, and when the charge is removed from it great quantities of black powder, containing copper, lead, zinc, etc., come away from it, and by putting in a feather to brush the zincs, much more can be brought away. This seems to have lowered the battery terribly. Its force at present is as follows: plates were rolled zinc.

2221. xliv. *My* 56 *pr.* 5 *inch plates, old, common papers, glass sides.*

Charge, W. 200; S. A. 4·5; N. A. 4. Action good at first, soon fell—on for 30′.

Gas in Large V. E. 28·5 c: i: = 3·614 grs. of water decomposed.

Whole charge 287 c: i:; 10 c: i: gave 93 gr. oxide, so the 287 c: i: contained 2669·1 gr. oxide or 2141·953 metal = 38·25 loss per plate.

As 3·614 W. is equivalent to 13·05 Z. nearly, so each plate has lost 2·931 equivalents, or the whole 56 pr. 164 equivalents nearly.

2222. xlv. *Same battery after rincing off black powder as much as might do.*

Charge, W. 200; S. A. 4·5; N. A. 4. Action on for 30′.

Gas in Large V. E. 29·5 c: i: = 3·74 gr. of water decomposed.

Whole charge 280 c: i:; of these, 10 c: i: gave 94·4 gr. oxide, so the 280 c: i: contained 2643·2 gr. oxide or 2121·168 metal zinc = loss per plate 37·88 gr. zinc.

As 3·74 W. is equivalent to 13·505 Z., so each plate has lost 2·805 equivalents, or the whole 56 pr. 157 equivalents nearly.

See xlvi and xlvii, expts. of 9th Jany. 1835, for comparitive results.

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2223. The production of a current by induction in an endless wire is a good case of proof that the electricity is previously in the wire, or at least is not derived from any thing external, though it may be excited by external means, i.e. supposing electricity matter, etc. and not merely motion.

2224. Joined the similar ends of helices i and ii (2107, 2140) to each other and to thick wires with large copper handles, for the purpose of obtaining a shock, etc. from helices only. The two helices of course acted as one having a wire twice as thick as that employed in either helix. Used with this the single pair electromotor. Could imagine I obtained a shock when contact was broken, but it was very weak. Easily obtained a powerful one in the tongue and mouth.

2225. When Iron was put into the helix then it gave a shock up to the arms.

2226. Then used 10 pr. Wollaston plates, 4 inch square, instead of a single pair. Now obtained shock both on making and breaking. The shock on making was from electricity direct from the battery. Query whether that on breaking was from cessation of this current, or from the reverse and induced current. On making the helix complete communication, then laying hold of the handles and finally breaking contact, there was no shock on laying hold, but there was on breaking contact.

2227. Must have a helix of thick wire and then can do all this better.

2228. Consider Marianini's effect of return shock, etc. etc. in frog's body, etc. May be due to the intensity of current on cessation.

2229. Introduced a platina wire at the cross wire of Electro magnet and arranged it as to length and thickness so that it should not be ignited when circuit through the electro magnet was complete. It was indeed of such diameter that no length, however little, could be ignited whilst circuit in the E. M. was complete.

2230. But on making contact the wire was ignited at that moment because of retardation for the instant through the E. Mag. circuit; the temperature then fell, and it could be so adjusted that either making or breaking contact should ignite the wire—whilst continued contact should not.

2231. The platina wire used was not very thin. Too fine a wire does not do so well as a coarser one.

2232. Tried same expt., using helices i and ii arranged as above (   ), but did not succeed for very plain reasons. The wires of the helix, because of their small diameter and great length, conduct so badly that a full inch of the platina wire introduced at the cross wires could be sustained red hot by the portion of electricity passing through it; and a quarter of an inch could be retained white hot. Hence it was impossible to distinguish the first effect from the constant effect.

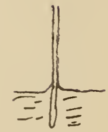
2233. Again, as the constant current kept it so hot, it was equally impossible to observe the effect of breaking contact. All these experiments require that much electricity should go through the long circuit and little through the cross wires—except at the moment when contact is made or broken.

2234. Used the E. Magnet and put the Galvanometer into the cross wires (   ); observed the constant deflection; then blocked the needle there, broke contact, and then on *making* contact observed the extra effect due to the greater current through the cross wire at that moment. Obtained the effect, but it was not great; principally because of the distance of the poles of the needle from the convolutions of the galvanometer and the distance therefore at which the power was acting.

2235. Made the same arrangement, using the helix i and ii (   ); obtained the deflexions both for making and breaking contact,

but they were very feeble, because the part of the current through the cross wires was great and the other part through the helix small. Try with a helix of thick wire.

2236. Repeated 2194 carefully, using the same piece of wire to make contact and complete communication in both cases. The running fire of spark is not so easily obtained with the E. M. as with the short communication (in this instance about 8 inches), apparently because, as current rises in former case slowly, there is no sudden repulsive action, either mechanical or electro mag., to drive off the mercury at *moment* of contact. With short wire there is sudden current; therefore sudden heat at point of contact and also sudden production of forces Electro magnetically to produce separation. In my old expt., at moment the current passed through the wire, the mercury was depressed around it, and a similar effect would take place here, aided by the momentum the mercury would receive. All these causes would be less effectual with the Electro magnet because of slowness with which the current there attains its full force.



2237. On tapping the cup containing the mercury, successive sparks could be obtained with the Electro magnet communication. I remarked that these sparks were not so bright as those obtained when the end was fairly dipped into the mercury and taken out again. This would depend in part on the difference of surface of contact in the two cases, the small surface in the first instance diminishing the quantity of electricity which passes; and in part perhaps upon the *time* required for the Electro magnet to change its state, i.e. to acquire its highest state on *making* contact and its lowest on *breaking*. Any deficiency in this respect would, by diminishing the quantity of passing electricity, diminish the spark on breaking contact. The diminished effect is, I take it, principally dependant upon this point of *time requisite* for the changes.

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2238. Have made an excellent helix of copper wire, $\frac{1}{5}$ of an inch in diameter. The wire in the first or inner helix contained 37 feet in length; that in the second and superposed helix 42 feet in length, independant of the connecting terminations. These helices

were united into one helix of 79 feet of wire in a consistent or conformable manner, and then experiments made with it and a single pair Electromotor.

2239. This helix gave a good spark on breaking contact.

2240. The two helices in one gave a better spark than either of the helices alone, so that the length was advantageous.

2241. Copper handles were fitted to the extremities and these held in hands moistened in brine. A shock upon breaking contact *was* now obtained by myself, Anderson and Mr Daniell. The shock on the tongue was very strong.

2242. On putting a platina wire, perhaps $\frac{1}{40}$ of an inch in diameter, at x at the cross wires, an inch or more was ignited, and retained so by the part of the current which then passed there. Took therefore a very much finer platina wire, so fine that a quarter of an inch of it could not be retained permanently red hot by the current of continued contact. Then tried effect at making and breaking contact. Could obtain ignition on making contact but not with this wire on breaking contact. A finer would, I have no doubt, give it; for the spark at breaking contact did not occur at the place of disjunction and hence the return current must have passed x.

2243. The ignition on making contact is liable to be produced in part by the state of the acid, etc. against the zinc plate, consequent on the momentary intermission of action; but I believe that the principal part was due to the retardation at first making contact.

2244. Now put the Galvanometer at x in the cross wire circuit. An immense portion of the electricity passed through the galvanometer, so that the needle was almost at right angles to the wires. So there was no chance of obtaining the effect (increased) on making contact. Again, when the needle was forced back to its natural position, it was so strained mechanically that, on disjoining the circuit at the Electromotor, I could not be sure the reversed deflection, little as it was, was not merely mechanical.

2245. In the galvanometer expt., there are several things against obtaining results. The galvanometer wire itself is rather long and produces inductive effects, counteracting those of the helix—not being so long, a greater proportion of the current goes through

than of equal length, and so its inductive force is somewhat raised.

22 DECR. 1834.

2246*. Copper wire, $\frac{1}{23}$ of inch in diameter. Six lengths of five feet each, soldered at ends to piece of copper plate so as to form terminations, and these amalgamated. When this bundle was used to connect the electromotor, it gave but a very feeble spark on breaking contact; but the spark was sensibly better when the wires were held together, so as to act laterally, than when they were opened out from each other—thus shewing lateral action.

2247. Made a longer bundle of the same fine copper wire. There were 20 lengths of 18 feet 2 inches each, and the thick terminal pieces of copper wire were 6 inches long and $\frac{1}{5}$ of inch thick.

2248. When this bundle was bound together by string, the spark at breaking contact was distinctly better than when the lengths or strands were spread open; but the difference was not very great. Not so great as I expected from the action of a helix.

2249. When a length of 19 feet 6 inches of the thick copper wire $\frac{1}{5}$ of inch in diameter was employed, and the sparks from this and the other alternated, this gave decidedly the largest sparks on breaking contact. The Sectional area of the two is very nearly the same.

2250. A piece of zinc on the tongue and silver beneath. Whether connected by a long or a short wire I could perceive no difference. The sensation occurred on contact—continued during contact and did not increase at moment of breaking contact.

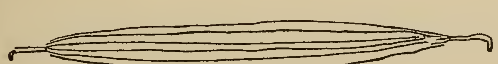
2251. A flounder used instead of the tongue: results the same—contraction on making, cessation on breaking. No difference between long and short wire (fine diameter and copper).

2252†. As regards bundles of wires, etc. If *a* and *b* equal in nature, diameter and strength, then equal inductive action in both and neither will tend to discharge the other.

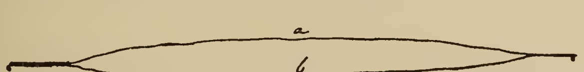
2253. If one much thicker than the other, still, as it will carry proportionately more electricity, the inductive effects will be proportional and neither will tend to discharge the other.

2254. If one made shorter than the other, its amount of inductive

* [2246]



† [2252]



force is then less and it gradually merges into the condition of mere cross wire.

2255. If the two, *a* and *b*, are of different quality as to conduction, then the results are not so evident, but probably equal lengths would be equivalent to each other in not discharging each other's currents; just as, in fact, they were found to be in my second series (201, etc.).

2256. See also 2218 of these notes.

9 JANY. 1835.

2257. My battery of 56 pr. 5 inch plates has been taken to pieces and rearranged. When dismounted, the papers were in good condition. The coppers were cleaner on the sides next the papers than on that next the zincs. Copper had been dissolved from the former sides: protoxide of copper had apparently been produced here and there on the latter sides. The zincs were in very bad condition, having a thick crust of deposited copper on them. This was generally as a soft black powder, but sometimes even in hard metallic scales which, when rubbed by a file, gave the red colour and lustre of copper.

2258. The plates were rubbed with a file whilst wet, etc.; brushed; and so this deposit of copper, zinc, etc. removed. They were then dried. There must have been great local voltaic action.

2259. The coppers have been covered with a coat of lacquer (shell lac dissolved in alcohol) on the outsides, i.e. those surfaces against which the paper would touch, and this allowed to dry and harden for two or three days.

2260. The box of the trough, having been left for two or three weeks, has become dry. The plates however were repacked as before, with papers between the coppers. Some of the papers were single thicknesses of foolscap which had been dipped in the solution of shell lac in alcohol and dried. Others were single thickness of cartridge paper dipped in a similar way; and others, cartridge paper not so dipped. 54 plates were reintroduced.

2261. On putting water into the battery, the box was found to leak all round the lower edges much, from the warping of the wood. The bottom is only pinned on, but this mode of junction

will certainly not do. This water in the cell enabled the battery to decompose water slowly in the Volta electrometer.

2262. Must try different boxes. Must also try to soak wood in mur. lime, etc., to prevent warping.

2263. The charge for this trough at present is 280 or 285 cubic inches of fluid.

Now made two experiments with the battery.

2264. xlvi. *My* 54 *pr.* 5 *inch plates. Newly arranged, lacquered coppers, glass sides, etc.*

Charge, W. 200; S.A. 4·5; N.A. 4. Action good: soon over: on only for 10′.

Gas in Large V.E. 45·9 c: i: at 37° F. = 5·82 gr. of water decomposed.

Whole charge 281·5 c: i:; 10 c: i: gave 87·5 grs. of oxide: so the whole 281·5 c: i: contained 2463·1 grs. of oxide or 1976·6 grs. of metal zinc = to 36·6 loss per plate.

As 5·82 W. is equivalent to 21·016 Z., so each plate has lost 1·74 equivalents, or the whole 54 pair 93·96 equivalents.

The probable reason why the loss of metal appears greater here than in the next expt. is that the plates were, of course, oxidized on the surface the first time of using, but clean and metallic the second.

2265. xlvii. *Same battery—second time of use after re-arrangement.*

Charge, W. 200; S.A. 4·5; N.A. 4. Action on for 10′; then nearly exhausted.

Gas in large V.E. 45 c: i: at 37° F. = 5·71 grs. of water decomposed.

Whole charge 281·5 c: i:; of these, 10 c: i: gave 83·6 oxide of zinc, so the 281·5 contained 2353·35 gr. of oxide or 1888·56 metal zinc = loss per plate 34·97 gr. metal.

As 5·71 W. is equivalent to 20·6188 Z., so each plate has lost 1·69 *equivalents*, or the whole 54 pair 91·26 equivalents nearly.

Vast difference between these and the former results (2221, 2222).

2266. Mr Clarke shewed me to-day several of his results of shocks, etc. obtained by his Magneto Electric machine. Most of them are related to the Magneto Electric Induction which I have

given in to the Royal Society. He thought there was a shock both on making and breaking contact, but I tried by a cup of mercury, etc., and found there was *none* on making—only on breaking contact.

2267. References as to electrical state of particles of matter.

Pouillet—Ann. de Chimie, xxxv, p. 401.

Davy—Thomson on heat and Electricity, 535.

Fechner—Quar. Jour., xxvi, p. 428.

15 JANY. 1835[1].

2268. Within the last week have observed twice that a slight obscurity of the sight of my left eye has happened. It occurred in reading the letters of a book, held about 14 inches from the eye, being obscured as by a fog over a space about half an inch in diameter. This space was a little to the right and below the axis of the eye. Looking for the effect now and other times, I cannot perceive it. I note this down that I may hereafter trace the progress of the effect if it increases or becomes more common.

16 JANY. 1835[1].

2269. Have renewed the investigation of *fluorine*, etc. Have worked to-day with some fine *green fluor*, very uniform and pure; very phosphorescent.

2270. 38 by weight of *fluor* and 152 of *sul. lead* (being equivalents), were pulverised and mingled together. A portion being heated on plat. foil in air, fuzed together at fair red heat and seemed to become quite clear. On cooling it became white and opaque; the platina was not hurt. Being pulverised and boiled in distilled water, the latter, filtered, contained no S.A.—no lime—no lead, etc. There had been no decomposition or useful result. I think the fluor had dissolved in the melted sul. lead, but am not quite sure. It might be in it as borax is in oil—invisible.

2271. A similar mixture (not fuzed) boiled in water in a flask for 40′; filtered, the clear liquor contained no sul. lime—no decomposition.

2272. Hence cannot obtain a *fluoride of lead* this way.

2273. Fluor 38 and Carb. lead 126 (being equivalents), were mixed and heated together. The carbonate became oxide of lead, but no further change occurred. Dilute nitric or acetic acid dissolved the oxide of lead and left the *fluor*.

2274. *Chloride* and *Fluoride of lead*, about equal volumes, fuze and mix perfectly, and apparently in very variable, if not in all proportions, for no line of demarcation between the two substances

[1] In the MS. these dates are written 1834, presumably in error.

before stirring was visible. This probably a useful mixture in voltaic decompositions for fluorine.

2275. *Chloride of lead* and *Fluor spar* do not fuze and mix together at such dull red heat as the chloride of lead will bear in an open platina capsule. There seems to be no action between the two substances.

2276. *Chloride calcium* and *fluor spar*, i.e. chloride and fluoride of calcium, fuzed together. The latter wets easily in the former and I think perhaps a part may dissolve, but not much. On treating the mass with water the chloride of lime only dissolves out.

2277. Tried expts. for a *fluor glass* for a stopper to apparatus.

2278. All fluor glass want the heat of the Oxy-alcohol lamp. *Alumina or clay* makes a more fusible mixture with *fluor spar* than silica.

2279. One in Vol. of white porcellain clay and One in vol. of pulverised fluor fuze pretty fairly; the resulting solid is white and opaque.

2280. Two clay and one fluor were more fusible. The mixture boils at one time in parts and yet becomes solid and difficultly or imperfectly fusible at a latter period: this is probably from action of oxygen on exposed substances.

2281. Two fluor and one clay not so good as equal vols.

2282. Fluor alone fuzes into a good knob and I think will perhaps do best. It makes a clear glass until the oxygen and heat acts on it.

2283. The plan will be, not to powder the fluor but to heat it carefully in a crucible, and then select a piece large enough and fasten it to the wire on which the plug is to be fixed. Once heated, the fluor does not decrepitate a second time.

2284. The fluor I think breaks into fragments because of the evolution of some gazeous matter, blowing it to pieces. Must heat it in a close tube.

2285. Passed Mur. Acid gas from S.A. and Mur. ammonia over fragments of fluor heated to bright redness in a platina tube. The gas at the end of the tube was principally Mur. Acid, but mingled with hydro-fluoric acid—as shewn first by its precipitating nit. of silver, which hy. fluoric acid does not, and by its acting on glass, which M.A. does not. But the action was not ready and good, so

that the process would not do well for preparing Hy. fluoric acid. The fluor in the tube had decrepitated into fragments and re-adhered by the heat. A little chloride of calcium was formed, but not much. The process of no use.

17 JANY. 1835.

2286. Heated some green decrepitating fluor in a glass tube, but did not obtain any decided appearance of action on the glass, though it was rather dull on the inside.

2287. Soluble *Fluoride of Silver*. Taste very strong and metallic; is quite neutral to litmus and turmeric paper.

2288. Sol. of *Fluoride silver* on glass did not act instantly unless warmed; and then did act. Being left at side of the bath, a crust of crystals formed on the drop which prevented further evaporation; this broken and the whole nearly dried. On washing the glass a black permanent stain was left, the crystals at the same time dissolving, except a thin film which looked a little like silica, but was dark; might be oxide of silver. Ammonia, being put upon the stain on the glass, dissolved the black substance. Nitric acid warmed did the same thing. In both cases the glass was deeply corroded and roughened. Hence glass suffers much and the black matter is probably oxide of silver—the oxides silica, alkali, lead, etc. being taken up in its place. It is probably a simple case of precipitation.

2289. A little sol. fluoride of silver on a good porcellain dish caused same deposit and acted in the same manner, corroding it deeply.

2290. A porcelain dish has had some solution of fluoride of silver left in it for some time past. It was much blackd.; and removing this by Ammonia and N. A., the dish was found much corroded.

2291. A little sol. *fluoride of silver* evaporated on platina gave *no black stain* against the metal, but a thin film of apparently brown oxide of silver was formed on the surface. When the evaporation was farther advanced, brownish but translucent crystals were obtained, deliquescent and resoluble in water. An[d] there was no action on the platina.

2292. By further heat these crystals become dry or solid, apparently without decomposition—but a little more heat evolves

acrid sharp fumes, and a fusible black matter is left, which crystallizes as it cools, giving a dull yellowish streak and being I believe the sub fluoride containing a little oxide. A piece of this, fuzed on the foil, being washed with water, gave solution of the soluble fluoride and left metallic silver. Being boiled with Water, the silver was very distinct by its ordinary character, brilliancy of streak, etc. etc.

2293. The vapour rising when the crystals of fluoride were heated may be either *fluorine* if the crystals were anhydrous, or *hydro fluoric acid* if they were moist, oxide of silver being then left with the sub fluoride: must ascertain this point.

2294. A piece of a silver capsule dried: it then contained silver and sub fluoride and also oxide. This in strong ammonia: the yellow sub fluoride changed colour, just as in water, and it is probable that ammonia does not dissolve sub fluoride as a whole, but produces the sil. fluoride and dissolves and combines with it.

2295. The ammoniacal solution warmed in air became black throughout. It was evaporated to dryness and left black solid substance. This, washed in water, gave a neutral solution and left the black substance having the general properties of my sub oxide of silver, which I believe it to be.

2296. The aqueous neutral solution gave plenty of silver with M. A. Being evaporated to dryness it gave a dark, solid substance, all of which redissolved in water except a little black film, perhaps oxide of silver. The solution, evaporated to dryness in *silver*, appeared to act on it a little, but gave a brown substance and not the *golden coloured* sub fluoride of silver. This solution is probably a compound of fluorine, silver and ammonia, or .

2297. Solution of *fluoride of silver* and ordinary *alcohol* put together—no change of appearance and both mix—being heated there was no change until the evaporation went so far as to produce the effects above with the solution of fluoride alone.

2298. Solution of *fluoride of silver* evaporated to dryness: then the Alcohol did not dissolve any thing, or very little—water dissolved it freely.

2299. Very *strong alcohol* added to the solution of the *fluoride of silver* and shaken—produced a brown precipitate this left in a tube to settle until monday. On monday the alcohol *had dissolved*

the fluoride. The precipitate of turbidness was not the fluoride. There was much action on the glass and deposition of oxide of silver, as with aqueous solution.

2300. Ether, put on strong aqueous sol. of *fluoride silver*, *did not* dissolve it or act upon it. There was plenty of action on the glass by the aqueous solution beneath.

2301. Solution of fluoride of silver in a tube with thrice its bulk of ether: no immediate action—shaken and left until Monday (See back—bottom of last page)[1].

2302. As silica and alumina precipitate oxide of silver from the fluate of silver, can probably pencil designs and obtain some good effects with it. Has its advantages above the nitrate this way.

20 JANY. 1835[2].

2303. Made fluoric acid from the large leaden retort—used the worst parcel of selected fluor—contained much earthy matter, not fluor, and much sulphuret copper, etc. The retort half filled with it and strong S.A., the latter swimg. over the pulverised fluor. Heated by a bath of solution of nitre with oil on top—used boiling temperature.

2304. Gas at first deposited silica against surface of water. In about an hour ceased to do so, and then collected the results, letting the end of leaden pipe dip a little way into the water contained in pewter vessels.

2305. First portion—good condensation—see heavy solution falling away from the beak of the retort—much sul. hydrogen comes over—water of condensation becomes yellow, turbid, hot and very acid. If water removed the vapours from retort very white and dense.

2306. Second portion condensed in water. A common pewter pot. As last, but no[t] so very turbid.

2307. Third portion in water in tin pot—as before. Nitre bath answers very well.

2308. Fourth portion. Some silica appeared and hence removed third water and put on the present—silica became more abundant. At one moment, on cooling bath, water went back into the retort

[1] i.e., see par. 2300.

[2] In the MS. this date is written 1834.

and evolution of gas or vapour ceased. After long continuing the heat, gas again appeared, but it was now fluo silicic gas almost entirely, and continued so to the end of the operation. This fourth portion kept apart from the rest.

2309. It seemed as if the water helped to bring on this evolution of silico fluoric acid, but it had appeared a little now and then before that.

2310. The operation was now stopped. It would be a good plan to have a pipe by which a current of air might be made to pass through the retort and so carry vapours forward. Is important also to use as pure fluor as may be.

2311. Now neutralized all the portions by ordinary Carb. potash: it took a good deal of this to do so. The *fourth* portion, being neutralized and filtered, gave very much silico fluate of potash and scarcely any soluble salt. It was therefore thrown away.

2312. All the others contained more or less silica, but as they all gave also fluate of potash they were mingled—the silico fluate of potash separated by filtration and the solution (still alkaline by excess of the carbonate) evaporated down to a certain degree to give a *rough fluate of potash.*

21 JANY.

2313. Opened the retort. State very good. Charge had swelled much and there was quite enough of it. It was hard and I think more S. A. would be good. By water, a chisel and care the charge was readily removed from retort without harm to the latter. The lead had not suffered.

2314. Would be useful to blow through—but a stirrer would do no good; the residue of Sul. lime is too stiff.

27 JANY. 1835.

2315. Have had a second distillation of Fluoric acid in the same large lead retort—a leaden tube with stop cock having been first fitted into the top, near the edge, to blow air in at.

2316. The fluor spar was the best picked portion—powdered moderately as before—not so much of the fluor, but more S.A., so as to cover it with an inch of the latter when all mixed together and left for a moment. The bath was as before, but placed over

the Laboratory fire and the operation worked in the Laboratory. It took the whole day.

2317. A little silicated fluoric acid came over at first, but it soon stopped, and then the thick fumes of Hydrofluoric acid with Sul. Hydrogen came over. These were condensed as before in the lead and pewter pots.

2318. The whole operation went on well—not much annoyance from fumes—very little—they condensed well in water—liquid always turbid from the sul. hydrogen acting on the lead or rather the tin dissolved by the acid.

2319. About middle of the day began to blow air through occasionally with a bag. Found it easy to do so with the mouth. It helps and does service and prevents all fear of absorption, i.e. it enables a quick remedy to be applied when, from the cessation of bubbles, there is fear of absorption. It should be directed downwards, and the inner aperture made smaller so as to mix the air better with the fluoric vapour.

2320. Perhaps if could obtain a little higher heat in the bath it might be better, but I am not sure; for as the residue cakes, it is possible it might blow it to pieces and do harm.

2321. The tin vessels are not good. The Hy. fluoric acid acts upon them, evolving hydrogen and producing, I suppose, a *soluble fluoride tin*—hence they are soon eaten into holes. The common *lead* pots are far the best.

2322. On saturating the first, second and third portions of acid received with *sol. carb. pot.*, there was very little precipitation of silica indeed; but there was some. Much Sulphuret of tin and lead was also then thrown down when the acid was neutralized.

2323. It will probably be needful to make it all into *fluoride potassium*.

2324. I think it would be good to mix the acid and fluor some days before the distillation, and stir well from time to time, so as to get a good decomposition first, and then at last distill off the fluoric acid. Perhaps passing steam through would help it.

28 JANY. 1835.

2325. Opened the retort this morning. At the bottom was a very hard cake of Sul. lime, etc., and over that much liquid oil of

vitriol, etc. The surface of the cake was exceeding hard. The acid above fumed much; it was poured off—the cake penetrated in the middle to the bottom—the retort rinced out two or three times and then filled with water and left for the stony mass to soften.

2326. I do not suppose more than a fourth of the fluor has been decomposed—if so much. I think well of the plan of mixing first for two or three days.

29 JANY. 1835.

2327. Have been working with the first product, i.e. that of the 20 Jany. It was concentrated in the alkaline state, having been over charged with carb. potash, and having been left for a night, it was found with some silico fluate of potash at the bottom and also crystals of Sulphate of potash. The super-natent liquor, having been poured off, was diluted somewhat, and then, being heated in leaden vessels, was almost exactly neutralized by the addition of some of the acid of the 27th—it was left very slightly acid.

2328. It was then filtered through paper in glass vessels (there being no sensible action in a short time when in this diluted and neutral and also cold state), to separate some further portion of sulphuret precipitated. In this state it was put aside in leaden vessels.

2329. A portion of this neutral (or very slightly acid) fluoride of potassium had sol. nitrate of silver added to it and the whole agitated. A black precipitate formed and the clear solution above contained no silver. Nitrate of silver was added to this clear solution, when a light brown precipitate was formed and separated. Again nitrate of silver was added and agitated, and now a fawn coloured precipitate was produced—whilst the solution also contained silver, and more nitrate did not produce any permanent precipitate. Hence three precipitates and a clear resulting solution was obtained.

2330. The first precipitate, acted on by ammonia, gave a solution of much chloride of silver, and a dense black precipitate (apparently sulphuret of silver) was left: this being washed repeatedly in ammonia and then dried and heated, was found to be metallic

silver. It had probably been sulphuret (precipitated), but was decomposed by the excess of ammonia.

2331. The second precipitate was divided in a similar manner into much chloride and a little of the black matter. The third precipitate was nearly all chloride of silver. All these chlorides were fusible, *unchangeable by heat*, etc. etc.

2332. Hence the Nit. of Silver had separated Sul. Hydrogen and chlorine and so cleared these things away. More was wanted than I liked. The chlorine has entered probably from the Carb. potash used, and perhaps also some in the distillation.

2333. Thought might use proto nitrate of mercury for this purpose instead of *silver nit.*, but the fluoride containing silver and so free from chlorine, etc. was abundantly precipitated by the Nit. mercury, because of formation of insoluble fluoride. Hence will not do—or not do well.

2334. Must examine the precipitated fluoride of mercury.

2335. Now precipitated the cleansed fluoride of Potassium solution by solution of nitrate of lead added in excess; an abundant white precipitate fell down of fluoride of lead. Water was added, the whole well mixed and left in Jars to subside. After a time the clear liquor was drawn off and examined and the precipitate put into a course of washing with distilled water.

2336. The clear liquor was evaporated in Berlin evap. basins and left on the sand bath all night.

30 JANY. 1835.

2337. This morning a small quantity of scaly crystals upon the surface and in the solution were found, which had gradually been formed. They were separated and washed but not broken up. When dry they were heated in a tube and boiled a little, evolving nitrous vapour in small quantity; and there remained a fluid which solidified on cooling, which could not be decomposed by heat alone and which by S.A. appeared to be fluoride of lead. The crystals were probably fluoride of lead, but mixed with a little nitrate which had not been washed out of them. Their quantity was small, hence nearly all the fluoride of lead precipitates.

2338. The liquor, carefully neutralized, threw down a white precipitate—there was not much of it. This was almost entirely

hydrated prot. oxide of lead, and resulted from the excess of nitrate of lead used in the precipitation. The liquor was previously acid because the nitrate of lead used was of course acid.

2339. A little further addition of potash rendered the liquor alkaline, and threw down a grey brown powder which was almost pure oxide of silver, resulting from the excess of nitrate of silver used in the first instance.

2340. This precipitation of the lead by alkali before the silver very remarkable. Probably depends upon the presence of some fluorine and the strong affinity between it and silver, but must examine this point. Silver, metallic, would probably separate lead from the fluoride and form sol. fluoride of silver. On the other hand, *lead probably not precipitate silver from the soluble fluoride.*

2341. Either the neutral or the slightly alkaline solution, upon concentration, gave crystals of Nitre and scarcely any thing else. Hence this a good mode of preparing fluoride of lead, provided the fluoride does not combine with a little of the nitrate of lead used and retain it during the washing. Must try this point.

31 JANY. 1835.

2342. A portion of the precipitated fluoride of lead which has been washed three or four times in cold distilled water now boiled in distilled water.

2343. The cold filtered liquid gave black precipitate with Sul. Hydrogen; another portion evaporated gave a solid matter, almost entirely *nitrate of lead.* This, dissolved in water, left a little insoluble which was partly sulphate and partly fluoride of lead: the sulphate was probably from the action of sulphates in the filter.

2344. *Same fluoride* boiled in a second portion of distilled water. The clear liquor blackened by S. H., and being evaporated, gave solid matter containing a little *nitrate* of lead and some fluoride.

2345. Same *fluoride* boiled a *third* time. The clear liquor precipitated black by Sul. Hy. On evaporation gave some solid matter containing very little, if any nitrate; it fuzed, etc. etc. and was *fluoride* of lead.

2346. Boiled a *fourth* time, gave solution blackening as before and giving by evap. a little solid fluoride of lead.

Boiled a *fifth*, *sixth*, *seventh* and *eighth* time: each time the

solution contained fluoride of lead. Now the fluoride nearly all gone.

2347. Boiled a *ninth* time: nothing left but a little insoluble matter, perhaps sul. lead.

2 FEBY. 1835.

2348. Examined the large retort (2325): the soaking in water has not softened the charge. Used a chisel and hammer and got out the charge very well, without hurting the retort. The bottom all sound, but a little leaking at the top where the joint not lapped over. Otherwise all well.

2349. The large quantity of Fluoride formed on the 29th Jany. (2335) has contracted much in the water since last week. As it must contain nitrate, boiled some distilled water and added it to the precipitate, agitating well by blowing through and thus washing well in hot water. Hope thus to get out the nitrate or the greater part of it. In the evening filtered it out. It was now very small in bulk, but heavy.

2350. Must precipitate by an acetate and hot.

4 FEB. 1835.

2351. The hot water washing of the fluoride, being filtered and evaporated, gave much nitrate of lead. Hence necessity for hot washing.

2352. Washed the fluoride twice more in hot water, filtered and evaporated the last washing: it gave very little soluble. Therefore put the fluoride on a filter, dried it on chalk stone and at last on sandbath. It is apparently a good *fluoride of lead* (by Nit. lead from the first distillation of Hydro fluoric acid in large leaden retort).

2353. Tried to combine finely powdered litharge directly with the hydro fluoric acid (a part of that of second distillation). This process will not do. It requires great excess of the oxide and warmth to neutralize all the acid, and then there is no means of separating the fluoride from the oxide.

2354. The use of carbonate of lead might perhaps not present this or other objections if, with a little excess of hydro fluoric acid, all the carbonate could be converted into fluoride.

2355. Worked upon the remaining portion of *sol. fluode. potassium* of the first distillation (20 and 29 of Jany.; 2312, 2329). Used Proto nitrate of mercury to form the first precipitates; and much sulphuret and chloride mercury fell down, which was separated. Continued the further separation by using nitrate of silver until in excess. Then filtered the solution and left it slightly acid all night in a glass Jar.

5 FEBY. 1835.

2356. This morning a thin crust of minute crystals had formed on the sides of the jar. It looked like the result of action on the jar, but when the solution was poured out, the crystals were easily displaced by a feather; and being collected together, formed a small collection of sandy looking particles, which, washed two or three times in water, seemed to give up very little by solution. The crystals were dried and remained unchanged in the air. Being put into a tube with a drop of Sulphuric acid, much gas was evolved, fuming in the air; smelling fluoric; producing no action or dullness on the glass *in* the tube, but on reaching the mouth of the tube, acting there by virtue of the moisture in the air. The glass became quickly dull, and even a platina plate, held against the mouth, became dull. Whether this gas is sil. fluoric acid and what the salt is, I must leave to be resolved hereafter.

6 FEBY. 1835.

2357. Took a part of the solution freed yesterday from muriates and Sul. Hy. and precipitated it by solution of *acetate of lead*. The acetate was made hot and the sol. of *fluoride potassium* poured into it, the acetate being in excess.

2358. Washed the precipitate thrice with boiling distilled water, agitating by blowing air through at the same time. Evaporated the third washing and found scarcely any thing left. Filtered out the fluoride and put it to drain and dry. It appears to be *good fluo. lead* (and is by acetate from first distillation).

7 FEBY. 1835.

2359. Evaporated the rest of the solution of *fluoride of pm.* (20 Jany., 6 Feb.; 2312, 2357) in a leaden pan; the silver in solu-

tion was thrown down by the lead in a metallic state. When much concentrated and in a half solid state it was put into a leaden vessel, covered over and labled.

2360. This fluoride is *very deliquescent* and soluble. The solution acts on turmeric paper as an alkali, even when slightly acid. The solution acts on litmus paper finally as an alkali. If reddened litmus paper be touched by *acid solution* of fluoride pm., it will *dry blue*; the blue will be restored. The hydro fluoric acid alone does not do this. The effect is perhaps due to the acid on the litmus paper taking alkali from the fluoride pm. and the hydro fluoric acid flying off.

2361. Hydro fluoric acid acts on turmeric paper as an alkali, i.e. it renders it brown as boracic acid does.

2362. Completed the neutralization of that part of the second distillation of the 27th Jany. referred to (2322), leaving it slightly acid and using heat. The process was performed in leaden pots. Filtered.

9 FEBY. 1835.

2363. This morning precipitated the nearly neutral sol. Fluoride potassium by Nitrate of silver added in excess. Separated the black precipitate and filtered. Evaporated the liquor in a leaden basin. The silver in excess thrown down.

2364. Fluoride of Silver does not stain the hand as nitrate does.

10 FEBY. 1835.

2365. Reference. Dr Davy on Sil. Fluoric acid in Jameson's Journal[1], about 1834 or 1835.

11 FEBY. 1835.

2366. Evaporated the second batch of *fluo. Potm.* (2362) and put it by in a leaden vessel; there was a good large quantity of it, perhaps a lb. in weight solid, but not fuzed and dry.

2367. Sol. Fluo Potassm., even if acid, put on litmus (reddened) or on turmeric paper, restores the blue of the first and reddens the turmeric whilst *dry and warm*; but if the places be moistened, the blue becomes red as by acid and the brown yellow, so that this curious effect depends upon temperature and dryness.

[1] i.e. *Edinburgh New Philosophical Journal.*

2368. Put together in a leaden vessel 1000 gr. finely powdered fluor spar, or 1 Equiv., with 2500 gr. oil vitriol, or 2 Equiv., and 2500 of water. Stirred them together and left them at common temperatures to act on each other.

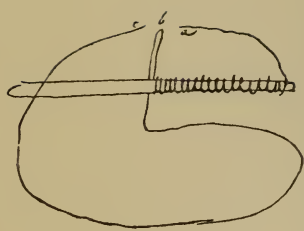
17TH FEBY. 1835.

2369. Fused portions of the two fluorides of lead in a platina crucible. They fuzed well; gave only a little fume as chloride of lead would do, and did not act on the crucible, i.e. that from nitrate did not and that from the acetate did slightly. Perhaps this results from minute quantities of the nit. and acetate being retained in the precipitate; if so, that from acetate best for my purpose.

2370. That from nitrate, on solidifying, was at first pale fawn; as it cooled it became darker; then red brown; brown, and when cold, *black*. That from the acetate was at first a pale green, but darkened and became at last *black*. Different order of colours to those generally produced by the action of heat.

18 FEBY. 1835.

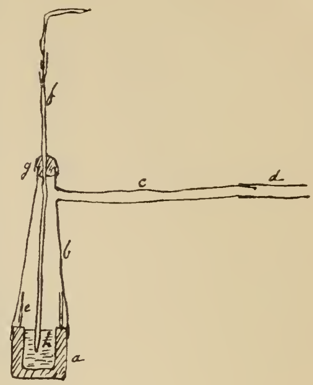
2371. To ascertain whether helix really better than straight wire of equal length in producing the inductive spark on breaking contact: *a*, *c* was one wire of 76 feet length, copper and of inch in diameter; *b* is the middle of that wire. The portion from *b* to *a* was made into a helix round a stick of wood, and the part from *b* to *c* left extended; *a*, *b* and *c* were amalgamated. *b* was then connected with one of the cups of a single pair of plates, such as those used before, and *a* or *c* were used to complete the circuit. Thus the current was the same through equal lengths of the same wire, and no doubt as to difference of wire could arise.



2372. When the current was through *a*, *b*, breaking contact gave a good spark; when through *b*, *c*, breaking contact gave a much less spark in every respect, i.e. as to sight and sound. The same was the case if *a* or *c* were continued in and contact broken at *b*; in which case the termination was the same.

2373. When *a* and *c* were both in one cup and the contact broken at *b* in the other cup, it was not so good as with *a* and *b* alone. Hence proofs of the value of the helix in this respect. I now have no doubt of its superiority over a mere extended wire.

Fluorine.



2374. Arranged an electrolytic retort thus: *a*, a bored plat. vessel; *b*, a top to it of plat. foil, soldered by gold to *a*; *c*, a platina pipe; *d*, a second prolonging pipe; *e*, a coil of plat. foil within to become neg. to end of *f*, and prevent injury to *b*; *f*, a plat. wire fixed by a plug *g* of green glass into *b*; *h*, fluoride of lead. All was tight, the end of the tube *c* or *d* being the only exit. By an argand spirit lamp the retort could be made full red hot, so that the fluoride within was perfectly fuzed; then the external end of *f* was made positive, and the retort *a*, *b* made negative by 20 pr. of Wollaston's plates, a volta electrometer being included.

2375. Plenty of gas was evolved in the Volta electrometer, shewing the passage of the electricity, but there were very little signs of any gaseous or vaporous matter being produced in the retort, for very little effect was produced. There were traces of acid fumes which reddened litmus etc., but they were very small. At last put a drop of water at the end of the tube *c*; it closed the passage and shewed that nothing particular was going on within, i.e. that no gas or vapour was evolved. There is a current through therefore, but either there is no decomposition, or else the wire touches the retort, or else fluorine combines or in some way disappears.

2376. Allowed the retort to cool below dull redness. Still the current passed, as was shewn by the Volta electrometer. When however the retort was quite cold, it did *not* conduct a current of Electricity. Hence can be no metallic contact within.

2377. The retort was ignited for about half an hour. It looked exceedingly well on the outside afterwards—that part of the arrangement therefore successful.

2378. The part *b* of the retort was then cut open so as not to disturb the state of things within. All was right as to non contact of the wire *f* with the retort *a*. There had been great action on the end of *f*, as if fluorine had dissolved it, and there had been action by alloy of lead on the platina *a*, but there were no signs of free fluorine any where.

2379. Now examined the habits of the fluoride of lead in the opened platina vessel. Found that, when cold, it did not conduct electricity as a current (the V. Electrometer being the test); but

that, when heated, it acquired conducting power even whilst solid, so that sparks could be taken against it and much gas was evolved at the V. Electrometer. It then acted as sulphuret of silver, for the current heated it more highly and then it conducted more powerfully. When fuzed, it conducted exceedingly well, and I could see no difference at the V. Electrometer, whether the wire merely touched the liquid fluoride or dipped through it and touched the platina retort beneath.

2380. When liquid, and the platina wire is dipped into it (not in connection with the battery), there is no particular appearance. When the platina wire is rendered positive, then the liquid round it is hotter than elsewhere, and there is a rapid ascending current, but no gas or vapour, or so little that it amounts to nothing. The current is only from the extra heat produced there.

2381. By far the greatest portion of the electricity passes as through an undecomposable conductor, a metal for instance; but there appears to be a little action, for lead is rendered at the cathode and the platina wire is corroded at the anode. The platina wire alone, i.e. without the current, is not corroded in the same manner.

2382. In this state this body presents an extraordinary case between ordinary electrolytes and ordinary good conductors. The sulphuret of silver is a similar case. Curious that heat *increases* conducting power.

2383. The form of the retort will do, but I must look out for a fluoride not having the peculiar properties this possesses. Try several.

20 FEB. 1835.

2384. Repeated the experiments on the conduction of the hot solid fluoride lead. Verified the results. It begins to conduct freely at below visible redness in day light, conducts more as it becomes hotter and conducts very well when melted. It is the case with the fluoride of lead both by Nitrate and acetate.

2385. As the first fluoride of lead (1403, 1405) gave gas at P. Electrode when electrolyzed, suspected now this was from nitrate or oxide present left in from nitrate when washed. Added a little pure oxide of lead to this pure fluoride of lead, and now gas *was*

evolved at the Anode. But this probably oxygen from the oxide. This confirms my view.

2386. Experimented with *plumbago Electrode.* Must heat the plumbago slowly first to redness, or else vapour of water forms in it and bursts it to pieces.

2387. This plumbago electrode in the fuzed fluoride of lead (pure) produced much gas when made positive, though pos. platina would not. Found on further trial that the plumbago acted in the same manner when *no electric current* was passing. Hence some direct and peculiar action of plumbago on the fuzed fluoride.

2388. A small piece of *heated box wood charcoal* plunged into and entirely under the surface of the fluoride produced the same effect. The fumes which rose were very acid to *litmus paper*, affecting the eyes and the nostrils by their acridity, and fuming much with ammonia. The carbon did not seem to diminish sensibly, but of this I am not sure.

2389. A diamond at the end of a platina wire, on immersion, did not produce any apparent action. But being continued in the fluid a few moments, it fell out of the wire, as if diminished in size, and afterwards when removed was, I think, much smaller.

2390. This effect surprized me much, and I am in doubt as to the result. I must examine whether its having lain at the bottom of the fluid in contact with the platina or the reduced lead did any thing. The diamond *sunk* in the fuzed fluoride of lead. The diamond appeared also altered and dissected on the surface.

2391. Some of the *nitrate fluo. lead* was pulverised and mixed with some recently ignited and powdered box wood charcoal. These were heated in a green glass tube and soon evolved much *fumes*, as of a gas, *clear*, in the retort but acting on water in the air. The gas was *heavier than air*—reddened litmus—fumed much with *ammonia.* Sent against a moist surface, it left a white coat of Silica, as if it contained Silicated fluoric acid. Being sent into a damp tube, so as to act upon water there and the air of the tube, shaken with a little water to remove all soluble acid, and then transferred into a clean tube and tested by lime water, *Carbonic acid* was found.

2392. Hence there is great reason to believe that a *fluo-carbon*

had been formed, analogous to *fluo-silicon* and acting in the same manner on water.

2393. On breaking open the retort tube, the glass was found acted upon, and hence probably the source of Sil. But lead had been reduced in the middle of the mass and quite away from the glass, apparently by the action of the carbon only.

23 FEBY. 1835.

2394. *Fluoride Zinc.*

Sols. fluo. potm. and sul. or mur. zinc do not precipitate each other when neutral and cold. Hence *fluoride Zinc* soluble probably.

2395. Hydro fluoric acid acts on metallic zinc, evolving hydrogen and yielding a solution tasting nearly neutral; this evap. on the zinc in a platina crucible, then dissolved by water and a little more of the H.F. acid transferred into a platina capsule.

2396. Part of this solution evaporated on a glass plate: it left crystals, small, apparently not soluble in cold water and scarcely when heated in it. The glass beneath was corroded and the crystals perhaps contain silica, etc. Being heated on a fresh part of the glass to dryness, the crust formed was crystallized, parts of the crystallization being fresh as if solution had happened. Water added to remove the crystals shewed that a little corrosion had gone on in the new place. The sol. and crystals evaporated a third time to dryness produced scarcely any action on the glass this time. The crystalline crusts did not deliquesce or at all tend to become moist.

2397. Another part of the sol. *fluo-zinc* evaporated in the platina capsule. Soon after warming it, acid fumes seemed to fly off and insoluble crystalline films formed on the surface of the liquid, before any important concentration had taken place, as if acid was necessary to dissolve the fluoride. Being evaporated to dryness, there was left a hard brittle crystalline mass, not resembling common preparations of zinc, tasting in the mouth. When this was heated on platina foil over a spirit lamp, acid pungent fumes arose and a white matter like oxide of zinc was left. There was a tendency to fusion in the central parts, but not in the outer portions. I suspect the water vapour had decomposed the *fluo-zinc*,

giving oxygen to the zinc and hydrogen to the fluorine. In close vessel the fluoride probably fuzed and do for an electrolyte.

2398. *Fluo-Tin.*

Strong solutions of *fluo-potm.* and *proto mur. tin* cause a coagulated precipitate, but water redissolves it. *Nitro Mur. Tin* cause no precipitate. Hence the *fluo-tins* probably soluble.

2399. Hydro fluoric acid acted only feebly on tin, evolving a little hydrogen. A little proto compound formed, but no useful result this way. The solubility of the fluoride of tin is probably the reason why tin pots and vessels will not answer as receivers, etc. for hydro fluoric acid.

24 FEBY. 1835.

2400. The *fluo-zinc* in plat. capsule had attracted water during the night and was damp. Being dried again on sand bath it became white and opaque without giving acid fumes. This, heated in a glass tube, gave but little fumes at the extremity, but a moisture condensed in the glass tube above the heated part, and a fusible portion (containing now however a little silica or silicon probably) was left at the bottom; for it was white dull opaque and fixed. At first the matter boiled much, giving off the condensible fumes.

2401. A portion of the above dry *fluo-zinc* in a platina tube, close all but above at a small aperture. When heated slightly, gave abundance of acid, pungent, acrid fumes in the air above. These soon ceased, and then the tube, heated to whiteness, suffered no further change. Being cooled and opened, fuzed yellow matter was found remaining, but it had boiled up in the tube and run about in all directions. The platina itself was not acted on. This fixed matter, being inclosed in fresh plat. foil and heated, fuzed again, evolving no fumes; hence it is fusible per se and is probably the same as that left in the glass tube. Being heated in the open air, it gave acid fumes and left oxide of zinc; the fumes probably hydro fluoric acid.

2402. The fumes from the plat. vessel and the liquid in the glass tube probably the same. Whether they are a volatile fluoride of zinc, or a hydrated compound, or an oxyfluorine or any other body, requires expt. to determine. *They are worth examining.*

2403. The fluo-zinc does not seem to change by being moistened

and again dried if in platina vessels. It at last is quite soluble; the solution was styptic, slightly acid to litmus and did not act with it and turmeric as *fluo-potm.* does.

2404. Being put on glass and evaporated, *there was action* on the glass; crystals such as those before described were formed, and now the nature of the substance is changed; probably a silico fluate.

2405. *Oxide of Zinc* dissolves in *hydro fluo. acid*, producing warmth; and a solution the same as that produced by the action of the acid on metallic zinc is obtained.

2406. There are no results here useful to me in my attempts to obtain fluorine by Electrolyzation.

2407. Tried some bodies besides *fluo. lead* as to their capability of acquiring conducting power by heat alone. Two troughs were used and a V. Electrometer included in the circuit. The conduction sought for was such as the V. Electrometer could shew.

2408. *Diamond* ignited highly did not conduct.

2409. *Fluor spar* made white hot did not conduct.

2410. Fluoride of Potassm., heated, did not conduct until upon the point of fusing, and then there is action and the platina at the anode is strongly acted upon.

25 FEBY. 1835.

2411*. Have had a retort of copper made, bored out of a single piece and with a copper tube and plug to fit air tight into it. The size of the apparatus was about that here sketched[1].

The retort and tube were heated to clear out all oil and moisture. Then 400 gr. of fuzed fluoride of lead and 20 grs. of coarsely powdered and ignited plumbago were pulverized and mixed in an iron mortar (there being very nearly one equivalent of each body); and about two thirds of the whole put into the retort. The retort was closed, suspended in an iron cage in a crucible furnace, the beak of the retort put into a pneumatic trough and heat applied. All was tight, and a little gas came over, but it was very small in quantity, and though the temperature was raised to very bright yellow heat, still no further evolution took place.

2412. After continuing this above an hour (urging the fire with

[1] The reproduction is one half the size of the original drawing.

* [2411]

bellows, etc.), the operation was stopped, and when cold the retort examined.

2413. The gas which came over was very slightly affected by water; a little of it was carbonic acid and the rest carbonic oxide. Very little fluoric matter was present.

2414. The retort was perfectly tight. It was cut open about the line *a*, *b*. All was clear through the tube, and the inside very clean. The contents had sunk down a little, but had not fairly fuzed; in fact, the touch of a knife easily removed the middle part; the side portions were harder. The whole was black, but the plumbago soiled the hands and in fact there was no appearance of action.

2415. It would seem as if carbon had *no* power over the fluoride in close vessels, and that the action before was due to the presence of water from the alcohol flame (2392).

2416. Now made a little platina retort out of foil soldered with gold and put several folds of platina foil inside to receive any action of reduced lead. Put some of the charge from the copper retort into this vessel and heated it, as before, by the Spirit lamp (2374). But now no gas evolved, agreeing in this respect with the result above.

2417. When this was cooled and opened, there was signs of extensive action on the platina. The vessel was eaten through in several cases and much of the platina was alloyed with lead. Hence there had been an action which does not occur when carbon is not present, for the fluoride then has no power on the platina.

2418. In the experiments (2387, etc.), I observed that when the carbon was introduced, the whole surface of the fluoride of lead boiled, evolving gas; and that after the carbon was taken out, the action continued for a little while. This looks like production of a compound of the fluoride with carbon, which compound could, in contact with the vapour of water from the lamp, evolve hydro fluoric and carbonic acid gases.

2419. Must try all this again, using less charcoal and using a dry atmosphere, and also quite submerging the charcoal. The solution of the diamond would be in accordance with this kind of action, and even the non-production of gas at the moment, i.e. until the

new compound formed had accumulated and could arrive at the surface. The action is in every point of view very remarkable.

27 FEBY. 1835.

2420. Melted fluo-lead in open plat. vessel (2384) over spirit lamp. Introduced a piece of plumbago *beneath* the surface. There was decidedly action and bubbling at the surface. All appears as if some carbon taken up by the fluoride. The action at the surface may however depend upon the surrounding aqueous atmosphere and therefore not go on in close vessels. The plumbago was very little diminished.

27 APRIL 1835.

2421. The reason why hot iron decomposes vapour of water so much more freely than cold is probably not because its affinity is greater for oxygen, but because at high temperature oxygen can penetrate the iron and travel inwards, something in the manner carbon does in making steel. Consider this in relation to the progression of particles from one to another by heat—in the voltaic circuit, etc. etc. etc.

2422. Examine an electric spark retarded by water by Mr Wheatstone's apparatus.

2423. Why does ether decolour a weak solution of sulphate of Indigo?

2424. If a freezing solution be well agitated during solidification, the ice ought to give pure water.

2425. Query the possible action of metals touching in one part and air only intervening in the others.

2426. Zodiacal light, theory of.

2427. Wine in a metal (pewter) cup continually rises in the thinnest films up the side to half an inch or more, and there accumulating, falls down again in continuall drops or streams. Is this an action of surface and will it lead to shew that, on using different fluids, there is some proof of an elective action, i.e. an influence depending upon the different nature of particles acting, not chemically, though in contact?

6 AUG. 1835.

2428. Can there be such things as double electromotors or double electrolytes, i.e. can a binoxide be a double electrolyte to a current excited by a double oxidation? This is not likely. The Electro chemic powers appear to find their standard of equivalency and possible existence in the quantity of force stored up in the elements of water or any other ordinary electrolyte.

2429. A hollow helix of copper wire, and thin filings or turnings put into it when the current is passing, would be a good general illustration of the force of aggregation.

2430. Remarkable that particles of liquids and gases do not seem to touch each other, as instanced in their not conducting heat, but yet evidently touch solids, as is shewn in their giving or taking heat from them. Must try oil over water, Mercury under water, etc. etc.

11 AUGUST.

2431. A striking exception to the general law of conduction of electrolytes is afforded by hot fluoride of lead, which conducts freely without decomposition.

2432. Probably many other bodies do the same.

2433. In such, the electrolytic intensity will probably have some relation to the temperature.

2434. Supposing that the effects of mixture could be avoided, should not the following effects be produced in a greater or less degree when platina electrodes are put into a solution of such a salt as sulphate of Soda? Should not these electrodes, after the first moment, be entirely surrounded by the respective bodies *acid at the P. E.* and *Alkali at the N. E*? Then no further decomposition of the salt ought to go on—so that what really does suffer after that should be such as mixtion carries to the electrodes. Hence a limit of action.

2435. Provided always the liquid itself does not conduct electricity of such intensity as can decompose. If it does, then a new condition arises which requires to be developed as to its principles.

2436. As sulphur is an *anion*, hence probably a reason why S. A. is not decomposed—both elements are anions.

2437. Hence perhaps also a reason for Boracic acid. Probably a general rule hidden here.

2438. Then Sul. Hydrogen may perhaps conduct and decompose—also other compounds of sulphur and boron with *cations*. Sulphuret of carbon: query its condition.

2439. Will bodies not giving conducting power when in solution, metallic poles being employed, give any power with other poles: do not think they will.

Very tired. Cannot get energies up.

6 SEPTR. 1835.

2440. Rose tells me that Berzelius in his annual account objects to my antimony proto sulphuret, and I am therefore hastened to its examination the first thing this autumn, having meant to defer it awhile before Rose told me this.

2441. Ordinary sulphuret of antimony fuzed at a dull red heat in a flint glass tube acts slightly upon it at the surface of contact, probably because of the lead in the glass. But when cold it separated pretty well from the glass, and I do not think the action would sensibly affect an experiment with weights. The sulphuret had the usual crystalline bright white appearance.

2442. Ordinary sulphuret antimony fuzed for about half an hour in a tube of green bottle glass had excited scarcely any action upon it, and the separation was better than before. The sulphuret was still unchanged.

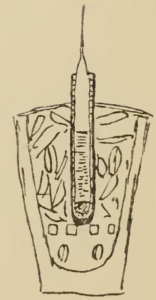
2443. Metallic antimony fuzed in a flint glass tube at a dull red heat and did not appear to affect it; it easily separated when cold and was unchanged by the contact of the glass.

2444. Ordinary sulphuret antimony and a little sulphur fuzed in a flint glass tube—the sulphur being of course as an atmosphere about the sulphuret. The sulphuret became cavernous from a little vapour rising from it. This might be from sulphur first dissolved, or perhaps rather from sulphur reducing the oxide of lead in the glass, producing sulphuret of lead and sulphurous acid, and this might be the gas or vapour: for there were many bubbles between the sulphuret and the glass when the whole was cooled and separated. The cold sulphuret appeared to be quite unchanged

in quality and no sulphur seemed to disappear. The action on the glass would probably not take place in green glass tubes.

2445. Then ordinary sulphuret antimony and antimony (metal) were melted together in a flint glass tube. Both were in lumps and no other means of mixing excepting shaking the tube a little was used. When cold, both the metal and the sulphuret separated well from the glass tube and the latter seemed scarcely at all touched. The metal separated also very well from the sulphuret. The sulphuret was no longer, as before, crystallized in needles, but had the peculiar dull steel gray appearance, and seems to be distinctly a new sulphuret. Thus far the trials bear out the former results. Now for experiments by weight.

10 SEPTR. 1835.



2446. A green glass tube was sealed at one end: 300 gr. of finely pulverized sulphuret antimony and 100 gr. of finely pulverized metal antimony were mixed well and put into the tube, and then the open end of the tube drawn out into a very fine neck with a minute aperture at the extremity. This tube was put into an iron tube (made out of 6 inches of gun barrel) with dry sand between the two; placed upright in a crucible furnace heated by a charcoal fire and retained at a red heat (within the glass tube) for 32′. It was then taken out of the furnace and allowed to cool.

2447. When cold it was broken open. The metal button and the sulphuret above it separated very well from each other.
The metal weighed . . . . . 54 grains. The sulphuret collected weighed . . . . . . 339 grains, but a good deal adhered to the glass: probably . . 7 grains more. Hence the sulphuret 300 grs. *had taken* 46 gr. *more of metal.*

2448. Repeated this experiment a second time in the same manner, except that the fusion was continued for *one hour*; but with the same results. The substances used were 300 sulphuret antimony; 100 metal antimony; and the tube weighed 147·5 grains. After the hour's ignition the whole weighed 547 grains, having lost only half a grain, probably moisture adhering to the powdered bodies.

2449. The button of metallic antimony weighed . . 52·5 gr.
the sulphuret therefore weighed . . . . 347·5 „
Hence the 300 gr. *had taken* 47·5 *more of metal.*

2450. Now 173 ordinary sulphuret contain 2 prop. antimony $= 2 \times 62{\cdot}5$, and 3 prop. sulphur $= 3 \times 16$. But if 300 take 47 more of metal : : 173 would take 27·1 metal, which is little more than $\frac{2}{5}$ of a proportional, so that, looking at it as combined and not merely dissolved, it would be 2·4 prop. metal and 3 prop. sulphur. This looks as if the latter portion was merely dissolved, but shall now see what muriatic acid does with the sulphuret.

11 SEPTR. 1835.

2451. Pulverized 250 grains of the new sulphuret finely—boiled them in pure muriatic acid—a metallic powder evidently left undissolvable—poured off the solution and boiled the residue in more M. A.—after this, but little sulphuret was left unattacked—again poured off the clear liquor. Now washed the powder with water and Mur. acid of such strength as to cause no precipitation.

12 SEPTR.

2452. Continued the washing this morning. Then weighed two equal filters—filtered out the metallic powder—it equalled 28 grains when dry and contained but little sulphuret—being melted in a glass tube, it gave a good button of metal. Assmg. that $250 - 28 =$ the ordinary sulphuret $= 222$ gr., which the acid had decomposed, then 300 ordinary sulphuret and 37·8 metal would have resulted from 337·83 gr. of new sulphuret. This does not agree with the former result, for here 300 ordinary sulphuret leaves only 37·83 metal by the action of acid, though it has taken 47·5 by fusion, etc.

2453. Still, my former conclusions appear to be wrong, whatever the cause of this difference may be.

25 SEPTR. 1835.

2454. Proto chloride of platina, prepared some time ago and well washed, has been left in water ever since, and now decomposed by sol. caustic potash, added until considerably alkaline. The cold alkali had little action; warming it, the powder became black and the alkalinity disappeared; added more alkali and heated as before —added a third portion and heated. The solution now *remained alkaline*. The black precipitate was now washed in several suc-

cessive portions of hot water and allowed to subside from them. This took several days.

2455. When the proto chloride was prepared the coloured liquor obtained by washing it well was preserved. It was probably a solution of the proto chloride in a little perchloride, and had a dark brown colour and was slightly acid. To this was added nearly neutral solution of fluoride potassium (containing a trace of lead in sol., but no Mur. acid). Warmth produced a black precipitate very like the former, and this was heated in water and washed several times.

2455[1]. A recent solution of perchloride of platina with sol. fluoride potassium gave yellow insoluble potash precipitate. Nothing useful in procuring fluorine.

5 OCTR. 1835.

2456. Some of the prot. oxide of platina which has never been dried (2454) put into dilute Hydro fluoric acid—but no action after long time. Put into dilute Mur. acid it soon dissolved, giving the dark brown solution of proto chloride.

2457. Prot. oxide of platina put into Nitro-fluoric acid, warmed and left all night—still no action.

2458. The sol. of Hydro fluoric acid used was strong enough to attack glass instantly.

2459. Leaf gold and leaf platina were put into Nitro-fluoric acid in platina vessels—warmed—and left for some days: but there was no action.

2460. Leaf gold and leaf platina were put in the same way into strong nitric acid, to which crystals of fluoride potassium were added, so as to obtain a strong N. fluoric acid—but no action after many days.

2461. The precipitate (2455) was dried on sand bath. Being then heated in a glass tube, water rose: this was removed as well as could be by bibulous paper—then more heat applied—more water rose, and also acid fumes acting on the glass—and also oxygen gas—the residue was metallic platina with a little lead (from the fluoride of potassium solution). The black precipitate may be principally fluoride of Platina acting on Water by the heat and

[1] No. 2455 is repeated in the MS.

so giving oxygen, Hy. fluoric acid and Platina—but I think it is more like the black oxide of platina with a little fluoride mixed or perhaps combined with it. It is not useful for my purpose because of the water held down in it.

16 OCTR. 1835.

2462. Amalgamated Plate of Zinc in a flat basin with dilute S. A. over it and a minute piece of platina wire on it. Gradually bubbles appeared over the whole of the Zinc and *near to the Platina* as well as *far off*. Very little gas rose from this small piece of platina, as it rested by its weight only on the zinc. When pushed into contact much more rose. Probably a sensible distance between them.

2463. The power the platina appeared to have at first it soon lost. Much of this due to its assuming the particular Marianini state.

2464. A similar plate of zinc and a larger piece of platina, still very small, weighing about $\frac{1}{10}$ of grain—more action than before from the platina—presses by more weight on the zinc.

2465. A still larger piece of platina (wire) gave more gas from any one part than an equal part of the small piece, but force fell by degrees from assumption of Marianini state by the surfaces.

2466. A Zinc plate and on it copper filings. More gas from this copper than from similar platina. Copper too does not so soon or strongly assume Marianini state. Action continued. But still bubbles on the zinc approached quite close up to it.

2467. Should perhaps not expect that Plat. and Copper protect the zinc from bubbles, for what local power the acid (of considerable strength) may exert upon it, it may also exert locally when the transferable power is called into action. The two appear in a certain degree quite distinct.

2468. Have been thinking much lately of the relation of common and voltaic electricity: of induction by the former and decomposition by the latter, and am quite convinced that there must be the closest connexion. Will be first needful to make out the true character of ordinary electrical phenomena. The following notes are for experiment and consideration.

2469. Does common electricity reside upon the surface of a conductor or upon the surface of the electric in contact with it? I think upon the electric, and must work out the results on that view. It will make a great difference in the collation and connexion of the various electrical phenomena and also in their explication.

2470. Would be a reason why all upon the surface of conductors.

2471. Present proofs: Excited Glass or resinous rod; charged state of glass jar in Leyden Bottle; excited state of Electrophorus plate; of air electrified in a room.

2472. Relations of this view to existence of Electricity on surface of conductors:

effect of form of conductor
condition and state of the electric
induction
excitation by friction or otherwise
conduction.

2473. With relation to existence of Electricity entirely upon the surface of conductors.

2474. Ascertain Coloumb's or Biot's fact with a globe about 6 inches or a foot in diameter, using a carrier that shall not disturb the form of the inside of the globe, i.e. a flat plate—not a ball. With this try the state of the globe at the side *a* or the center *b*, or elsewhere, to parts to which the little ball *b* may be bent.

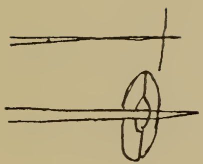
2475. Make a part of the globe as thin as gold leaf and try it there upon the inside.

2476. Try the inside of a cylinder with straight or curved ends.

2477. Try the bottom of a cup—or inside of a hemisphere.

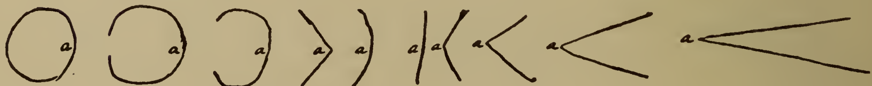
2478. Try both sides of a plane of large extent, at different distances from the center.

2479. As globe becomes larger and larger, air being within, any part in it ought to approximate towards state of a plane in open air. Try therefore very large globe within.
2480. Try same globe full of metallic surface, as leaves of dutch foil.
2481. Try same sized globe of solid conducting matter; probably contain less electricity than if thin and filled with air.
2482. Try a glass globe, one part being metal leaf or plate; this more capacity for Electricity in that part than a metal globe probably.
2483. Pursue the change from the inside of a globe to the outside, and onwards to a point even; thus, ascertain experimentally the state of *a* in the following series of forms and changes*.
2484. In relation to the electricity residing on the electric, consider the effect of increased or diminished extent of surface.
2485. In relation to the effect of form—in addition to the points above, trace minutely the influence of edge or point.
2486. Is not the whole effect due to the higher electric state which the electric at the point can acquire, and to its repulsion or attraction directly forward, in consequence of the gradual decrease of power backwards? In fact, is it not the electrified electric which rushes of[f] by its own relations, and not by any force of the point except in a secondary manner?
2487†. Observe effect of a point in a ball or in a tube.
2488. Open the ball or tube behind so as to allow fresh air to flow in: will not the point then resume its force? Will not smoke be drawn in by these holes behind? (See 4 July 1836.)

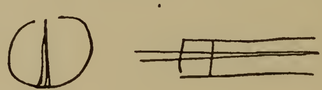


2489. Use a point with a screen of mica near its end and observe the current of air by the smoke of a green taper, etc. brought near it. (See 4 July 1836.)
2490. Action of many points together—a ball of points. (See 4 July 1836.)
2491. Examine action of points in liquid electrics, as oil, Oil of turpentine, etc., containing floating particles.
2492. Consider the state of the insides of the gold leaves in a Bennet's Electrometer. They ought not to be electric at all. Hence divergence cannot be the effect of repulsion.
2493. Gather together the facts which favour the idea of repulsion. I suspect there are very few, if any.

* [2483]



† [2487]



2494. Consider the state of all other surfaces which are considered as exerting a repulsive action. Are they not merely consequences of the impossibility of being able to charge conductors with any portion of electricity?

2495. Consider the supposed existence of Electricity upon the electric, and not upon the conductor, in relation to the *nature of that electric.*

2496. Thus, will the same conductor in different Electrics have the same intensity and quantity with the same quantity of electricity?

2497. A metal globe in Air; rare air; glass (a body so as to avoid what is called induction); Wax; Oil; Oil turpentine, and other electrics, electrified from the same common source, and then brought in contact with an Electrometer—the divergences observed. If the results are constant for the same body, but vary with different bodies, then a proof that the electricity is related to the electric, not to the conductor.

2498. The same with gases of differing density and qualities.

2499. „ „ rarefied air.

2500. The relation of common air, hot air, flame, etc. in this view. How does flame discharge? Will a flame in a tube discharge?

2501. Consideration of the phenomena of *induction* under the idea that electricity resides on the surface of the electric.

2502. Difference in quantity of electricity held by induction on opposite sides of electrics of different densities and natures, but of equal thickness.

2503. Relation of the two surfaces under induction.

2504. Is not the evidence of a surplus in one over the other only a consequence of the fact that other inductions besides the principal one exist, and that the very means used to detect this excess often supply the necessary conditions for these secondary inductions? Must guard against them.

2505. Consider the relation and dependance of the two surfaces under induction.

2506. Consider the conditions and states of an electric under induction in its various parts. Thus, at its middle parts, or more or less near to the sides.

2507. If a plate of glass could have its two surfaces placed in a mutual state of induction, and it could then be split into two plates, what would be the electric state of the two new surfaces? Would they not be highly Pos. and Neg.? If put together again, would not the electric state or tension of these two surfaces disappear *though still existing*, and may not the same be true of every other part of the glass between the two surfaces?

2508. Or suppose a plate of glass under induction, having a leaf of gold in the very middle of it, parallel to its sides. Would not the two surfaces of the glass in contact with the sides of that leaf be in strong P. and N. states? Yet if the gold leaf were away, and the surfaces of glass were together there, they would be in the same relation.

2509. Hence the state of the plate under induction is the same as the state of a Magnet, and if split or broken, would present new P. and N. surfaces before not at all evident.

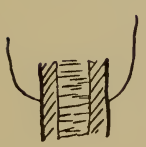
2510. Beccaria's plates.

2511. Now this is the state of an electrolyte in the circuit before that traversing of the particles has taken place by which the electric force is transferred and the body conducts. It ought to be the state of the electrolyte also whilst it is solid.

2512. Must try again a very thin plate under induction and look for optical effects, i.e. detect its polarized state.

2513. Place a plate of Mica under induction, then split it and examine the state of the new surfaces.

2514. Put Air under induction by two plates 1 and 2; then introduce a third 3, and examine and consider the state of the air surfaces against it. Or introduce a plate of Electric, as mica, and examine its surfaces. This mica probably shew something good. Will it assume a transient or a permanent state, or what will happen?



2515. Select if possible an electric capable of assmg. both the solid and the liquid state; put it under induction thus; then examine states and relations of the surfaces of the solid and fluid portions.

2516. Probable that phenomena of induction prove more decidedly than any thing else that the Electricity is in the Electric, not in the conductor.

2517. Induction appears to me to pass into the case of ordinary excitation when an electric or conductor is surrounded to a great distance by conducting matter, i.e. they are probably extreme cases under one general law.

2518. Query the ratio of induction between a small ball placed in the middle of a surrounding metallic globe with the same ball placed in the middle of a much larger sphere. Query also the connexion of Harris' laws with this part of the subject.

2519. If an electric can be actually charged, then as charging appears to be a different phenomenon to induction (though it may prove to be the same), to what depth can an electric be *penetrated* by the electric power or fluid from a conductor, either when induction is taking place or not.

2520. Endeavour to ascertain this with different electrics. Will a thin coat of Oil or wax or lac bring away all the electricity or will a little charge remain in the rest, etc.? In other words, what is the thickness of the plate of the Electric actually charged; is it sensible?

2521. Is there any necessary and essential relation of conductor and electric by which both are required in the phenomena of residence and induction? I think there cannot be in induction and therefore not in residence; but consider the point well.

2522. Can conducting matter be absolutely impregnated and charged?

2523. Can electric matter be thus impregnated and charged?

2524. It would seem in Air, oils, etc. that electrics could.

2525. What is the electric state of particles of charged air or of oil, etc. if electrics only can be charged?

2526. Is not air actually charged by the action of a point? May not the air of a room be charged? May not a jar of oil be charged? Or is the charge in this case all upon the surfaces of the mass?

2527. Try electrified oil by a searching wire.

2528. Are not brushes of electricity chargings of air? If they are cases of induction, search out the limits and forms, etc. of the inducing surfaces.

2529. If brushes are actual chargings of air, then trace and connect them with sparks and discharges.

2530. Query the quantity of Electricity which may be com-

municated to any given quantity of any one kind of matter, it being bodily charged.

2531. If electricity reside on the electric, then the phenomena may have relation to *particles* and not to masses, i.e. to the particles of the electrics which touch the conductor.

2532. The proportion of Electricity thus resident upon the surface of the *electric or conductor* must be an almost infinitely small quantity of that belonging to the particles by natural right and giving them their forces of chemical affinity. Work out this relation.

2533. In relation to the proportion of power and the mechanical effects it produces, consider Herschell's considerations upon the forces moving mercury between the poles in a solution.

2534. Consider these motions of mercury also as included in the mixed case of induction and electrolytic decomposition.

2535. Consider residence of Electricity on the Electric in connection with the phenomena of *excitation*, especially by friction.

2536. Do they not offer some of the best proof that the electricity resides in the electric?

2537. Excitation by friction cannot be due to ordinary chemical action, as Wollaston supposes, but is very probably due to a disturbance of that force in the superficial particles which is inherent to them, and which in its full exertion produces chemical phenomena.

2538. Proportion between this force (the electro chemical equivalent) and the minute quantity actually excited by the friction.

2539. Consider conduction in good, moderate and bad conductors in relation to the idea that electricity (free) resides upon the surface of *electrics* and not of conductors.

2540. Make a Leyden phial of a glass globe, charge it and seal it up hermetically, leaving the outer coating on or not at pleasure; then leave it for few or many days and examine its state with reference to discharge taking place through the glass, and its conducting powers being thus shewn.

2541. Does not hot glass distinctly conduct? Easily tried: the action of the air being quite excluded.

2542. If all electricity resides upon the exterior of a globe or mass of conducting matter, then what is the condition of our globe of the earth as a conductor suspended in air?

2543. Is it electric within, or not?

2544. Is it in a state of induction with respect to the circumambient medium, i.e. is the surface of the air next the earth in one state, the space above being in another?

2545. Is not the space above generally positive to the surface near the earth being negative?

2546. Do not the points on the earth, leaves, etc. etc. etc., serve to balance this state continually, and is there not therefore a general action here which may account for some general phenomena, as the growth of trees upwards, etc.?

2547. Is evaporation the reverse operation by which the state of the upper atmosphere is retained always positive? It is a process as general and constant in its direction as the discharge in the other direction is.

2548. Is not Gay Lussac's hanging wire from the balloon a proof that, even with regard to the earth itself, it is simply as a ball suspended in an electric; that the electricity resides in the air, i.e. the electric; and that the air is in a state of induction?

2549. If the globe and its surrounding air were actually electrified with a surplus portion of P. or N. Electricity, we should not be able to discover it, moving as we do upon its surface and having no other point of comparison than what it affords.

2550. Supposing the whole surface of the globe to be electrified up to the condition under which an extended plane surface gives a spark to the knuckle of a certain length. What would be the equivalent of the electricity so spread over it if expressed in grains of water?

2551. With respect to *Induction*. What is the relation of two electrics in contact when both are included in one act of induction? Carry this into the extreme case of glass and air, as when a Leyden phial is stripped of its outside coating. Consider also the mobility of the one electric or the other.

2552. If there be two fluids or powers, then is there any thing due in conductors to a necessary relation of two fluids, whilst in Electrics the relation need not of necessity be to more than one—or if to two, by the act of induction only.

2553. Make a new jar machine—a tube closed at one end—coated within—excited well without—coated without—and then

discharged. Perhaps get by induction a very useful instrument this way.

2554. The retardation of the middle spark in Wheatstone's three sparks is probably a connecting Link between conduction and induction. Must consider it in relation to induction in Metals.

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2555. Fluorine, etc. etc.

Some months ago I put a strong hot solution of fluoride potassium into a leaden vessel to keep. It became solid when cold, but being deliquiescent, a solution has formed above it which in warm weather is all fluid, but in cold weather deposits large crystals of hydrate fluoride pm. Action has taken place on the lead by the joint action of the solution and the air, and fluoride of lead is in solution and crystals of it slowly form as crusts on the surface of the liquid. To-day I find a curious galvanic process is also going on; for metallic lead, crystallized, is deposited in leaves all round the upper edge of the solution, in the solution and on the lead. Hence, though lead is dissolved in one part by this solution from the lead, it is deposited in another part from the same solution also on the same metal lead. This perhaps depends upon the greater solvent and [illegible] power of strong solution over weak solution for fluoride of lead. It is evident that the part of the lead in the solution near its upper surface is the cathode of the electrolyte, for there the lead is evolved.

2556. Sol. fluoride potassium kept in lead (air had access) had become adulterated by production of fluoride of lead which was held in solution. To free it from this, it was put into a leaden bottle and a current of Sul. Hy. gas (from sulphuret of iron and dilute Sulc. Acid) passed through it until all the lead was precipitated. The solution had been previously freed from Muriates, etc. by Nitrate of silver in former operations, and being now filtered through a paper funnel into platina vessel, was considered as pure fluoride of potassium.

2557. I required a solution equivalent to hydro fluoric acid in which I might render platina, Gold, etc. positive by the Voltaic battery: for this purpose I added strong sulphuric acid to the strong solution of fluoride Pm. The first portion caused an

abundant formation of crystalline solid matter. No fumes of H. f. acid were produced (unless large quantities were used), nor did the solution taste as sour as I expected. The second addition of S. A. increased the effect. The third produced a tendency to liquefaction, and acid fumes became apparent. The fourth rendered it more liquid and acid, and I now considered it fit for my use as a solution of hydro fluoric acid. There is sufficient reason to conclude that the sul. potash or any excess of sulphuric acid would not interfere with the proposed experiments.

2558. I suspect the first addition of sulphuric acid does not act by merely decomposing the fluoride of pm. in this state of strong solution, but either throws it down from solution, or combines with it or with some elements present so as to produce a new state of things, which is only subverted when more acid is added. This is worth enquiring into hereafter, and also the direct action of dry *fluo. pm.* with S. A.

2559. *Platina* made Pos. and Neg. in this strong solution of hydro-fluoric acid. Gas at both electrodes, probably O. and H. *Strong Electric smell.* There was no solution of the platina in a reasonable time—proved further by no precipitation of platina at the N. Electrode.

2560. *Gold* made Pos. in this acid solution. Gas at both poles, probably oxygen at the gold—but there was much solution of the P. Gold and instantaneous precipitation of it again at the N. Electrode of Platina.

2561. *Palladium* rendered Pos. in the acid: instantly acted on, dissolved and transferred to N. Electrode.

2562. *Silver* rendered Pos. in H. f. Acid: instantly acted on, dissolved and quickly transferred to the N. Electrode.

2563. *Iron*, *Tin* and *Antimony*, rendered Pos. in the Hy. fluoric acid, were acted on and transferred instantly in the same way.

2564. Copper rendered Pos. in the Hy. fluoric acid was affected in the same manner—with this kind of exception. If the battery was not strong, a black body formed on the copper and the electric current was stopped. The body produced appeared to insulate, and was probably a *proto-fluoride of copper* insoluble in Hy. fluoric acid. But if the battery was active, this crust often got broken up by gas evolved at the P. copper and then the

action continued. In all solutions of Chloride, fluoride, etc. there are two electrolytes in the current, and both may decompose at once: the decomp. of water would break up (mechanically) the results of the decomp. of H. F. Acid, and so produce the effects above. When the crust, etc. was broken up and action continued, then copper was dissolved and transferred to the N. Electrode.

2565. *Zinc* rendered Pos. in the hy. fluo. acid (strong sol.) would occasionally refuse to act, and current seemed stopped. This probably depended upon the formation of an insoluble fluoride, not necessarily insoluble in water, but in this strong solution of *fluo. zinc* formed against the P. Z. When the acid was more dilute the current passed. Zinc was dissolved at P. Electrode and deposited at the N. Electrode.

2566. The more oxidizable metals would of course act in the same manner, still more powerfully. Thus Platina appears to be the only metal with which the fluorine refuses to combine. If a compound of Plat. and fluorine could be obtained it would almost doubtless be decomposed by heat: but if the compound cannot be formed this way it is very unlikely it can be made *in any other*.

2567. I doubt Davy's result of a fluoride Plm. formed at the P. electrode. His Sil. acid may have contained silver or lead or Mur. acid and any of these would give false results.

2568. I now tried the metals in the same Hy. flu. acid with nitric acid added, considering this as equivalent to a *Nitro-fluoric acid* and analogous to *Nitro-muriatic acid.*

Platina rendered	Pos. in N. F. acid	–no action on it–no transfer.
Gold	Do.	–instant action and transfer.
Silver	Do.	– Do.
Palladium	Do.	– Do.

2569. The rest of the metals would of course act more readily but as the three last. Here again the resistance of Platina to combination with Fluorine under the circumstances is wonderful.

2570. May the power of Chlorine to combine in the same circumstances with platina, when fluorine does not, be taken as a proof of the greater affinity of *chlorine for platina* OR of *fluorine for Hydrogen* and similar bodies.

2571. Now compared *Platina and Gold* in relation to fuzed Fluo lead as a different fluoric electrolyte to H. F. acid.

2572. *Platina* made pos. in fuzed fluoride of lead—no sensible action on it during the time the neg. platina could stand, but that soon eaten through by the lead reduced there. Of course Air and water from flame had access, but still the absence of action on the P. platina is strongly illustrated.

2573. Gold made Pos. against fused fluoride of lead was instantly acted upon and run down by that chemical action—not by the head only. The fluoride appeared to mix with the fluoride of lead.

2574. I now made Platina and other metals positive in solutions of fluoride of *silver*, *zinc*, *copper*, etc., hoping that when the *cation* of the Electrolyte was also a metal the relation of the elements concerned might allow its anion to unite under more favourable circumstances with the *Positive Platina*.

2575. *Fluoride of Silver*: this solution has been made by allowing hydro fluoric acid to dissolve moist oxide of silver, since which it has stood over great excess of the oxide of silver for several months (   ) in a platina crucible. It is in an excellent state (   ).

2576. *Platina* rendered P. and N. in this solution of *fluoride silver*. If the action be strong the N. Platina evolves gas (hydrogen), and metallic silver in shooting acicular crystals rapidly forms there; if action is weak, metallic silver only appears. At P. Electrode there is occasionally, indeed always gas, probably oxygen, and acid fumes arise, probably due to the heat soon excited in small quantities. But besides these a black brittle crust forms on the N. Platina.

2577. This substance is insoluble in and is not altered by water. Being washed, it was dried on the sand bath to about 212°. Then put into a very small glass tube closed at the bottom, and further heated by a spirit lamp. A little water rose from it and there was slight appearance of action on the glass. It then fuzed into a brown-red fluid, translucent in small quantities, and not changing apparently at a heat able to melt the glass. The air or moisture of the flame had scarcely any access during the application of the heat.

2578. Put the solution of fluoride of silver on to a plate of fluor spar to get the Electrodes further apart. At N. Platina electrode, the usual acicular crystals of silver shooting across and soon

completing the circuit. At the P. Plat. Electrode, the black substance forming out in dendritic crystals (like some corals in appearance) going across to meet the silver but not growing so fast or so fine. It must be a conductor therefore like the silver, or it would not increase at the ends. It can hardly be a compound of the platina, for that would not have such a form; the body is in fact produced at the end of the previous portion and where no platina is. It seems like a body separated out of the electrolyte, and is most likely some per compound (fluoride or other) of silver, which yet conducts.

2579. When the P. Electrode with black body and N. Electrode with silver on it were *changed sides*, so as to make the Black body the N. Electrode and Silver the P. Electrode, then the *black body rapidly disappeared* before silver was produced at the N. Electrode; and the silver Positive disappeared, and during its disappearance prevented the Black body from forming at the P. Electrode. Looks very like a per fluoride of silver, which by heat probably fuzed into a proto or sub fluoride and then resisted heat as before (   ).

2580. Can hardly be a fluo platina compound. There was no signs of platina action.

2581. *Gold* rendered Pos. in sol. fluoride of silver gave the same general appearances and produced the same black compound; but I believe gold also was dissolved at the P. Electrode.

2582. *Silver* rendered Pos. in sol. fluoride silver presented phenomena parallel in their nature to those obtained with fuzed chloride of silver. There was rapid solution of silver at the P. Electrode and rapid deposition of it at the N. Electrode—no black substance was formed.

2583. When the action was more careful, so as not to evolve gas or vapour rapidly at the P. Electrode, then a black crust formed, something resembling the former black crust—except that it was not dendritic and appeared to insulate the silver, whilst it remain perfect on the surface. When broken up by the gas, etc. then rapid solution and action went on.

2584. *Copper* rendered Pos. in sol. fluoride Silver. Action violent; so much silver soon thrown down that current was carried by it and action on the solution ceased. I could not tell whether the black powder was formed or not, in consequence of the direct

action of the copper on the solution—but most probably it was not formed.

2585. Now experimented with fluoride of Zinc as the electrolyte. To make it, put H. fluo. acid, which had been retained in a leaden bottle, into a platina dish with zinc filings. So much fluoride of lead had been formed by time that scarcely any hydrogen escaped, lead in metallic state being thrown down as the fluoride of zinc was formed. Boiled the solution with the zinc; still little further action, and the solution at last had much acid power to litmus.

2586. I am half inclined to think zinc is not more soluble than lead in Hy. fluoric acid and that probably the compound analogous to the oxide would be insoluble. Perhaps that is the compound which insulated in the former experiment (   ).

2587. *Platina* rendered Pos. and Neg. in the acid solution of fluoride of zinc. No hydrogen at N. Electrode, but the metal zinc deposited there. At the P. Electrode gas (oxygen?) was evolved, but no black powder and no action on the platina. On stopping the electric current, the solution redissolved the Zinc precipitated at the N. Electrode, evolving hydrogen; but no platina was left, hence none transferred from P. Electrode. Probable therefore that the black deposit in fluoride of silver is a compound of silver.

2588. The solution of fluoride of zinc was evaporated to dryness—it crystallized on the capsule—with water it dissolved entirely—the solution was acid. Hence perhaps this solution, and that of acid in the lead bottles, are those produced when direct action of the acid on these metals is allowed its full power.

2589. The solution, after ceasing to act on zinc, can still act on glass or silica.

2590. Added oxide of Zinc and boiled it with the acid solution; still it remained acid.

2591. A solution of fluoride of copper made with the per oxide, and blue in colour (therefore a per fluoride), had Platina rendered P. and N. in it. Copper was precipitated at the N. Electrode. There was no black compound formed at the P. Electrode nor any action of the fluorine on the platina there.

2592. Supposing fluorine obtained and held in platina vessels.

Would it not abstract hydrogen from water at common temperatures?

2593. Would it not inflame in hot steam? Would not a mixture of fluorine and steam burn spontaneously or be ignited by a flame or an electric spark?

2594. Would not fluorine serve as an abstractor of hydrogen, and so be opposed to bodies generally as abstractors of oxygen.

2595. *Electricity of tension*—residing on electric, etc. Consider a drop of water or mercury on a conductor and how and why it becomes pointed. Will it be so in Oil and liquid electrics? (See 4 July 1836.)

2596. Try to solidify a charged fluid Electric, as shell lac, etc.

2597. Why is non-conducting matter attracted, as it cannot assume opposite states in opposite parts?

Conduction.

2598. Conduction by metals and by electrolytes appear to be essentially different—are they really so?

2599. Heat and cold have probably an extensive influence over conducting powers of bodies, beyond what is at present known—even to an entire change of their character.

2600. Perhaps iron, highly heated, may change in its conducting power—perhaps gain or lose much. Try with electricity of low intensity.

2601. Try also nickle, cobalt, copper, lead, tin—sulphurets, phosphurets—infusible oxides—infusible metallic minerals. As sulphuret of silver is one case, there must be others. Fluoride of Lead is another.

2602. Try with electricity of a single pair of plates. This with iron is enough.

2603. Must trace conduction from discharge in air to discharge through metals.

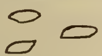
2604. Truth of case of conduction of spark by line of water or sulphuric acid.

2605. Will hot air confined in a tube discharge? Explode Ox. and Hy. in a tube: will the flame at the moment discharge a jar?

2606. Try discharge across Cagniard de la Tour's dense ether

vapour—across sulphuret of carbon vapour—M. A. gas—Mur. ammon. vapour.

2607. Wheatstone's expt. of middle spark retarded. If short lengths of water or dilute acid interposed between the sparks, should obtain the effect of a long wire.

2608. As the center spark is thrown back, a greater length, i.e. more duration, should be given to each of the sparks. Important to see whether they can be quite separated by an interval of time, thus , and also whether the middle one cannot be lengthened more than the others.

2609. Remember that there is no loss of power in conduction, whether it be through conductors or electrolytes, i.e. the amount of power is every where the same in the circuit.

2610. Decompose dilute S. A. in hermetically sealed tubes by wires, etc.

2611. Liquefy Mur. Ammonia in close tube under pressure. Is it then an electrolyte?

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2612. From here forward is Stevens' writing fluid—from this date backward for several pages is the writing fluid I bought in Long Acre.

Magnetic.

2613. It is scarcely likely that Iron, Nickel, Cobalt and chrome are the only magnetic bodies. It is far more likely that certain temperatures are necessary for the effect, and that these bodies are on the right side of the point of temperature required for each, whilst many others are on the wrong side.

2614. At what heat do iron, nickel, cobalt and chrome respectively cease to be magnetic?

2615. At what heat does protoxide of iron and loadstone cease to be magnetic?

2616. Does not iron, when it has lost magnetic power by heat, also lose its power of intercepting magnetic influence and so become like copper, silver, etc. (2881) 27 Jany. 1836.

2617. Must cool extremely other metals—sulphurets, plumbago,

charcoal, etc. etc., and see if some of them do not become magnetic. Cool also some oxides.
2618. Apply [illegible][1] mode of testing for the magnetic force.
2619. Probably none but conductors of Electricity need be tried —but it is possible that bodies may change in this respect by cold. See conduction notes.
2620. Consider Dr. Schmidt's facts as to the destruction of power by contact of *contrary* poles of magnet and preservation by similar poles.
2621. On intensity of a simple unaided Volta-electric effort and distance at which chemical affinity between particles may be supposed to act.
2622. Try to obtain V. Electric indications (beyond those already obtained) without contact (metallic).
2623. Bring the ends of a single pair of plates into very near vicinity by a micrometer screw, and examine the place of contact by a microscope, in day light and in dark also.
2624. Let the ends terminate in same metals as the plates, or both in platina or similar substance.
2625. Let the ends be two amalgamated drops—or two drops of brine—or sulphuric acid—or two long gold leaves.
2626. Examine these ends in air—in vacuo—in water—in Sulphuric acid or potash—in oil of turpentine, etc.
2627. Will zinc really preserve copper in brine, being nearly but not quite in contact with it—Davy.
2628. Ordinary contact can scarcely be called contact. Try a contact on one side with mercury, with filmed platina so that mercury *does not adhere*: that must be an ordinary contact—is it effective?
2629. Work out the developement of combustion as an electric phenomenon—its relation to the light and heat produced—their definite or indefinite proportion proved—causes of their variation.
2630. Doubt Davy's result of Acid and Alkali becoming Negative and Positive with metals by contact if no chemical action—if chemical action, believe both will become negative, for both have the same kind of action in exciting voltaic combinations.
2631. Will Platina be acted upon by solution of chlorine?
2632. Dry platina in dry chlorine. Will not dry platina in dry mixture of chlorine and hydrogen inflame them?

[1] Possibly "Haüy's".

2633. Electricity—its residence upon Electric or conductor, etc. etc.
2634. The inside of an electrified quart pot contains no electricity.
2635. The inside of a Leyden jar charged and standing on the ground *contains no electricity*—all is on the glass.
2636. Must coat a jar inside with gold leaf and try another gold leaf against it—or remove even gold leaf coating itself and see if it electric.
2637. Or make part of inside coating removable. It will probably bring away no electricity from a charged jar.
2638. The outside coating of a jar can shew signs of electricity only by inductive relation to the air outside as a second electric, the glass being the first; in fact, that coating relates to two electrics and two inductive actions at once.
2639. Suppose a globular bag of leaf gold—if electrified, it would swell out. How can this be repulsion, as there is no electricity on the inner surface?
2640. Expansion of gold leaf into a straight flat form, when electrified in the air, is a good result and shews well the disposition of the forces.
2641. What would be the state of an interminable flat plane or surface surrounded wholely by air and out of reach of inductive action of neighbouring bodies?
2642. I begin to doubt electrical repulsion altogether.
2643. If it does not exist, how greatly all electrochemical relations will be altered. It would knock a supposed force out of existence. If this true in nature, it would be a great good to disprove repulsion.
2644. Leaves of a gold leaf electrometer cannot open by repulsion, for when close together there is no electricity on the inner surfaces, and when furthest apart there is most.
2645. As to Leyden jar, when cap and ball on, that ball is electric; but only because it has relation through the air by induction to the outside coating.
2646. Have a ball in connexion with the inside of a charged jar, gradually raise it to the outside and approach it nearer and nearer to the outside coating. Observe its successive electrical changes.
2647. Can a metal surface take from the inside glass surface of

a charged jar–it being free from external and extraneous influence of induction. No.

2648. Does not all this prove most perfectly that electricity resides on the electric and not on the conductor–and also that common residence of electricity and induction are the same thing?

2649. The Electric must be in a state of tension throughout. That air in the interior of a jar or vessel cannot assume this state throughout, because of opposing actions on all sides, is probably the reason why no electricity there.

2650. Ought rather to look at surfaces of electrics than of conductors. Thus, convex surface of the electric, inclosed in a hollow metal globe, relates to the limited extent of that electric, whilst the concave surface of the metal *does not necessarily* do so.

2651. Must try the inside of a large insulated boiler at Maudsley's –or build up a large box with tin foil or sheets of copper.

2652. Try a large copper at Coppersmith's, Chandos Street.

2653. With respect to notion of *no repulsion*–remember magnetic effects, and also the other relations of two electric currents in wires, etc. etc., and the different indications of repulsive action arising out of such parts of science.

2654. The taking a surface of an electrified globe and leaving no electricity in it is the best proof that a mass of conducting matter can hold no sensible quantity of electricity.

2655. Consider the state of air or oil, etc. between plates of a condenser, where induction can be let down, as compared with the state of glass between plates, and glass of a jar, where it cannot be let down by removal of metal merely.

2656. Obtain a numerical result of the quantity of electricity on a given surface of air or glass, charged by induction or otherwise, as compared to the quantity in an atom or a small quantity of such bodies indicated by their chemical energies.

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2657. Must try experiments of induction, etc. etc. with Negative Electricity as well as Positive. As there is a decided difference in the sparks, flashes, etc., this must have some direct and important relation to the real condition of bodies in a negative state, and may lead to true relation of Positive and Negative.

2658. Is not excitation by friction a mere consequence of the separation of parts, previously in mutual chemical relation, but not combined, i.e. in that intermediate state of chemico mechanical condition which the sixth series shews bodies often have.

2659. Sliding, that is, rubbing, is the best mode of separation for leaving such particles in their strongest state of excitation.

2660. Rub two pieces of paper with india rubber, separate them by sliding, not by splitting—will the effects not be stronger?

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2661. I cannot go on at present with the fluorine experiments and wanted therefore to put away the fluoride of potassium prepared as before described (2556). Some of the solution has been retained in a platina tray; other portions in platina crucibles. In the solution appeared some small crystals, the largest $\frac{1}{4}$ inch long, prismatic. These washed, were tasteless; fuzed on platina, became black and contained I think fluorine, and also lead, but what else I do not know.

2662. The solution of fluoride pm. was evaporated slowly and carefully in platina crucible and then fuzed in Platina and poured out on Platina to cool. As it was fuzed, immediately after liquefaction, it began to boil and oxygen rose from it, just as if it were a chlorate that was decomposing. Perhaps there is a fluorate there, but I had not time to push the matter farther. I did not urge the heat until this ceased, but cooled the liquid mass on the platina plate, and when solid, put it bye, part in an alembic of platina and part in a glass stoppered bottle; but both were put away under glass jar[s] standing in basins and closed by mercury so that no air should get in.

2663. The fluoride of Pm. is very deliquescent (at least, the mixed salt just described); almost as much so as chloride of lime.

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2664. Have borrowed a copper from Mr Kipp. It is a new one, not quite finished, and having no cock fixed in it, so that its shape and condition is regular. It is made of three sheets of copper for the sides and one piece for the bottom. A row of rivets run round the bottom therefore and three rows from the bottom to the top. The top has a ledge or rather edge turned out in a horizontal direction. The copper is placed on an insulating stool on the middle of the Lecture room floor. A strip of thick flannel, with a drawing string at each edge, has been brought over the upper edge, thus , for the purpose of preventing dissipation there. Another flat band has been stretched over the rivets round the bottom. But I doubt if these are of any consequence. The drawing in section and dimension of this copper are given on the other side,[1] with the stool.

2665*. This copper was connected by a wire with a gold leaf electrometer, and electrified; then a carrying ball suspended by a white silk thread brought in contact with its outside, and afterwards brought in contact with another and similar (large) electrometer. The effect of this carrying ball was much less on its electrometer than that of the copper on the electrometer attached to it. Nor could repeated transits bring the former up to the latter.

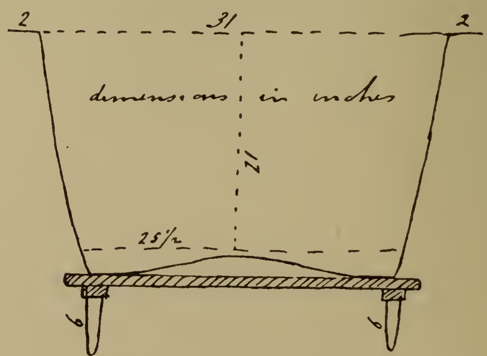
2666. Shall call the instrument connected with the boiler the *boiler electrometer* and the other the *test Electrometer.*

2667. Put the test electrometer on the ground, and used a carrying ball 10 inches in diameter, covered with tin foil, hung by white silk. Made the ball touch both copper and electrometer at once. Then the test Electr. opened as much as the other. But on taking away the ball from the copper, it (the *Test E.*) fell, though still in contact with its Ball.

2668. This is a natural effect of induction, for the surfaces of the copper and globe which are opposed whilst in contact have little

[1] i.e. on the opposite page of the MS. See figure, par. 2665.

* [2665]



or none electricity in them. After the globe is separated from the copper, the electricity in the former spreads over all parts, and consequently the intensity falls at the part in contact with the electrometer, or rather in the electrometer itself.

2669. Good to know this; there are many other cases of such influence and consequent falling, as when samples are taken by carrying balls from parts more or less under inductive influence of air around, or are taken by plates or by projecting portions of matter.

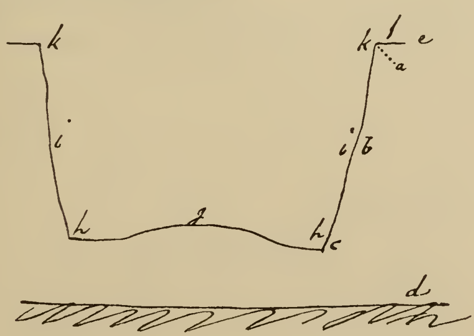
2670. When a carrying ball is in contact with an electrometer, a certain divergence is produced. If ball removed, divergence falls; if approached again, it rises. This a similar consequence. When parts separated, there is more free surface for general inductive action and therefore intensity falls; when approached, the surface between the ball and electrometer is thrown out of action and the intensity rises elsewhere.

2671. The indication given by a sample ball will never therefore rise as high as that of the copper Electrometer unless the instruments be peculiarly related, for of course Copper Electr. can be so placed as to be masked by copper itself, and then indications taken from parts under induction would surpass it.

2672*. Now examined the outside of the copper by a small carrying ball, about 2 inches diameter and covered with tin foil: the flannel was removed from the edge of the boiler and the examination was made first on the outside. It was least electrified at *a*; more at *b*, and more still at *c*. Much of the effect at *c* was traced to the inductive action of the opposed floor at *d*, about 6 inches below. At the edge *e* it was as highly electrified as at *c*; over *f*, or the flat ledge, it was less.

2673. In proof of the effect of inductive action at *c*, when a sample was taken from *b* in a fair way, it gave a certain divergence of electrometer: but if, whilst the sample was being taken from *b*, an uninsulated brass ball, or the hand, was held near to it, then the sample had 3 or 4 times the power on the electrometer to what it had before: according to the nearness of the hand or ball and the consequent effect of induction.

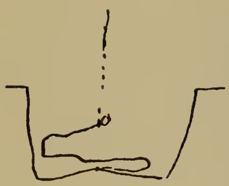
* [2672]



2674. A sample taken from the boiler electrometer cap was stronger even than that from *e*; simply because the effect of the wire and cap, etc. approached to that of a point.

2675. Now examined the electric state of the inside, using the same small carrying ball as before and avoiding all inductive action by use of long thread of silk. Obtained no traces of electricity from *g* and *h*; at *i* obtained a minute trace; at *k* most as to the inside, but that less than at *f* even. Hence none at *g*, *h*, or scarcely at *i*; some at *k*; more at *f*; much more at *e*, but little at *a*, increased towards *b* and downward to *c*. The angle formed by the edge of the copper there complicates the progressive effects.

2676. Now endeavoured to obtain state of a ball connected by a wire with bottom and lifted by a silk thread into different positions. It does not matter whether the wire touches the side or the bottom or the outside, but probably the form of ball or point may be influential. At the same time, it is the carrying ball that at the moment of observation constitutes the terminal part there. The ball was raised from the middle upwards; until $\frac{2}{3}$ of distance up it gave no electricity to the carrier. When a little below the level of the edges it gave a little. When level, it gave about as the inner edge, or a little stronger; then, as raised higher, it gave stronger and stronger until as strong as the electrometer attached to the copper. At that time it might be 20 inches above the level of the boiler.



2677. Put an electrometer inside the boiler and connected its cap with the boiler by a wire. When boiler was electrified, this electrometer was not at all diverged; but being raised and still continued in metallic contact by the wire, it gradually expanded as it rose above the edges into the open air.

2678*. Now wished to charge the boiler much higher. Put a conductor with a Henly's Electrometer in connexion with the boiler and found that, when boiler was electrified so as to open Gold leaves of Singer's Electrometer 70°, that the Henly was just affected; probably not more than 3°. Hence extreme proportion. Then removed the gold leaf electrometer, and charging a jar, touched the copper with it so as to electrify the whole up to about 45° of Henly; and removed the jar.

2679. With this comparitively high charge examined the copper

* [2678]

by the small ball as before. Still obtained none from *g* or *h*, and very little from *i*. From the rest obtained electricity in the same order as before, but much stronger in degree.

2680. Now connected the boiler by a wire with the Electric machine, so as to keep it as highly charged as I could, without much dispersion. Still none from *g* or *h*, or a little below *i*. The rest in the same relation as before.

2681. In repeating these experiments I used a large globe, 10 inches in diameter, covered with tin foil, but inside being wood, plaster and air. It of course much altered the form of the inside, but still from within near the bottom brought out no electricity. Gave some irregular results due to a cause to be explained presently (   ).

2682. Now cleaned two brass balls from lacquer by hot solution of potash and plugged in silk threads by means of wood. These are to be used as carriers and are one 3 inches diameter, another $1\frac{1}{2}$ inches. A brass plate was also cleaned in a similar manner; it was flat, round and 10 inches in diameter and slung by silk thread.

2683*. The boiler was now retained charged by placing a charged jar against it, and thus the Henly's electrometer retained well diverged for a long time together.

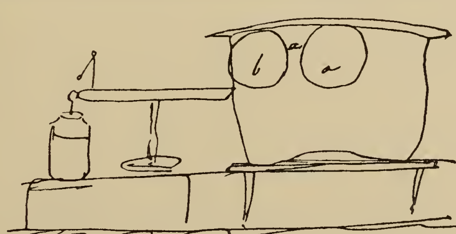
2684. The carrying brass plate applied any where against the inside at the bottom brought up no electricity with it. Applied against the side within and below, it brought away no electricity. Flat against the side, near the top, as at *a*, it brought away a little. The plate edgeways against the side near the top, as at *b*, brought away much more.

2685. The plate applied flat to the outside, about midway, brought away moderate electricity; very fair, as might be expected. Applied edgeways against the outside it was very strongly charged.

2686. Must remember that the carrying ball or plate forms for the time the surface and shape of the part where it is applied. Hence a plate and a ball very different.

2687. *Precautions.* Must take care of the effect of the air of the room, electrified by the machine, acting on the Electrometer. Would be better if the latter were in metal. The one I use as test

* [2683]



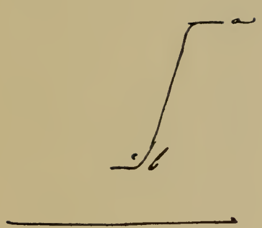
Electrometer is very delicate, and the faces of glass opposite the gold leaves coated extensively with tin foil within and without, both being [in] contact with the ground. The glass of an uncoated electrometer soon becomes charged.

2688. Must guard against the charge the carrying ball can receive by merely passing near or even within some inches of the edges or angles of the electrified boiler. If in the vicinity but for a moment only, being all the while fully insulated by a silk thread, still it becomes considerably charged. This electricity of the carrying ball is always negative if the boiler is positive, and is an effect of induction, the air apparently discharging the pos. Electricity of the ball. It is not therefore an effect of communication from the boiler.

2689. Whether the edges of the boiler be covered with the flannel or not, the effect is equally striking. It is greater opposite a point, as of a wire, than the edge of the boiler, but is still negative unless the ball (2 inches in diameter) is about or within half an inch of the end of the wire: then it is either neutral, or begins to be positive, having actually received electricity.

2690. This effect, with its progression, will vary with the fineness of the point, the projection of it from the boiler, and the charge of the boiler, and probably also with the diameter of the opposed carrying ball.

2691. The boiler was now charged by a jar as before ( ) with Negative electricity, the edge being from this time left uncovered, for the flannel seemed to be of no use. The carrying ball was the 3 inch ( ). The effects generally were as before: the least electricity was taken from the middle of the outside, and the quantity increased downwards and upwards, except in the angle at the top edge; being as strong at the extreme edge *a*, through the effect of form, as it was below at *b*, where induction also help, but form less favourable.



2692. On the inside, thought I obtained a trace of N. Electricity from the angle at *c*, which ought not to be. The other parts were just in the same relation as with positive electricity.

2693. When the carrier passed near the edge of the boiler it assumed a charged state by induction, and retained it just as when boiler Positively electrified ( ); i.e. a state of the

contrary kind was acquired by the ball in merely passing through the air near the Negative boiler, at its edge, etc.

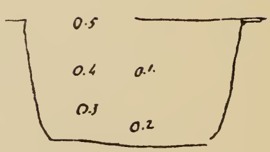
2694. It is hardly possible to go within a couple of inches of even a plane or convex surface without the ball assuming the opposite state—much more is the effect produced near edges or angles on the outside.

2695. With the small carrying ball the same effects are produced.

2696. *Extraordinary care is required also* that the hand should not deliver, or it or any other conducting matter touch the carrying plate or ball *in presence* of the electrified boiler, unless they are after that to touch the boiler itself. A carrying plate or ball 12 or 15 feet from the boiler (when electrified so that Henly diverged 36°), being touched, then insulated and finally examined by the delicate electrometer, gave proofs of a charge obtained at that distance by induction.

2697. The boiler was now covered by two sheets of copper and its inside examined, provision being made against the causes of error above in the following way. The carrying ball was held in by its silk suspender; the boiler covered over; the whole charged by the jar; the ball dropped against the bottom for a sample and again raised to the middle of the space within; the jar was then removed; the boiler discharged and opened and the ball examined by electrometer. It *always* came out quite unelectrified, whether it had touched the top or bottom or side within at any part.

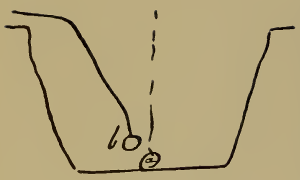
2698. Again, if ball put into the boiler unelectrified—the boiler covered over—electrified—discharged—opened and the ball examined—it does not become contrarily electrified by induction—nor even if the boiler be only half covered over, provided the ball be under the covered part as at 1 and 2; but at 3, 4 and 5 began to shew electricity of the contrary kind, and is therefore by induction.



2699. A point placed inside at the bottom of the boiler gave nothing to the carrying ball by contact. But when the ball was over it at 2 or 3 inches distance, the boiler being quite open, there was plenty of the opposite electricity by induction.

2700. The point being raised higher and higher, being still in connection with the boiler, it gave more electricity as the ball had done before (   ).

2701*. This case passes into the one of a point in a hollow ball.



2702. In using a ball or plate to take specimens of electricity by contact, the proximity of the hand, body or other conducting matter connected with the earth must be avoided, for then induction takes place even at the inside. Thus, if a ball in the hand be held at *b*, or be in any other way connected with the earth, the carried ball *a*, separated from the boiler before *b* is removed, bring[s] away much electricity; for at *a* inductive action towards *b* exists at the time.

2703. If the hand holding the silk thread by which the ball is suspended be 5 or 6 inches from the ball, this effect is produced in a serious degree. If at 2 or 3 inches distance, there is much effect. The same effect takes place at the outside also, but not so strongly. This all has relation to the exaltation of the intensity of the electricity of the boiler by induction at the part where the carrier is applied. The carrier of course brings away the *same electricity* as that of the copper.

2704. Must read on these subjects Coloumb, Poisson, Biot, Becquerel, Harris.

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2705. A cover not in contact with the copper will act by induction, if touched, like a ball connected with the earth: if not touched, as a ball brought near to the electrified copper (   ).

2706. Electricity must pervade metal, i.e. be in and through it, even in its quiescent or unmoving state, as when a solid metallic body is electrified—but then how attenuated it must be. Now it is clear that air as a mass has not a greater capacity; for a globe filled with air or with metal, and charged with equal quantities of electricity, has equal tension or nearly so. So that difference of air and metal, or of electrics and conductors, at their surfaces of contact, is not that former have more capacity than the latter, considered as masses.

2707. The true difference appears to consist in the electric becoming polar throughout, whilst the metal cannot. By that, especially under induction, is the capacity of the surface of the electric so greatly increased, whilst the character of the surface of the conductor as to capacity never changes.

* [2701]

2708. When this is more developed, must consider the condition of intermediate bodies, as bad conductors; by these pass to consideration of chemical decomposition, and also to *the extremes* of *insulation*, *induction* and *conduction.*

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2709. Repeated the experiments in a closed boiler (   ), with the large 10 inch tin foil ball, taking all precautions necessary (   ).

2710. When in contact with the inside of the covered boiler, lifted up, taken out and examined, there was no trace of electricity in it.

2711. When put into the inside, not in contact, and retained there a while whilst the boiler was charged, it acquired no electricity by induction.

2712. These experiments are in fact other forms of Biot's experiments with the globe and covers.

2713. Now proceeded to observe the *inductive effects* at different parts of the boiler upon an approached ball in contact for the time with the earth. These were very beautiful. The carrying ball was brought near the part of the boiler to be examined, then touched, then removed and then examined. It shewed of course contrary electricity to that of the boiler, the stronger as it was the nearer to the boiler at the moment it was touched.

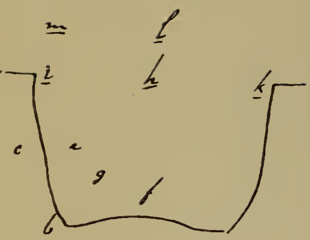
2714. A scale was put on to the side of the electrometer and then the ball tried in different situations, at such distances as to give always the same divergence to the electrometer. This distance was found to be greatest opposite those places *on the boiler which were least electric when examined by contact, and least opposite those parts which by contact were in the highest state of electrical excitation.* Thus the ball about the situations figured gave equal intensities of electricity.

2715. These positions of equal intensity of induction were then better ascertained by a wire of copper about $\frac{1}{20}$ of inch in diameter, bent double, thus*: the rounded end *a* being brought opposite the electrified surface. This acted as a small ball, and at a certain distance its intensity rose so high that electricity passed to it, producing in the dark a luminous brush and also an *audible*

* [2715]

brushing noise quite loud enough to be distinctly heard at once. These effect[s] did not come on slowly, but at once, marking distinctly the place at which the intensity by induction had risen up to this sufficient and constant degree, and thus supplying an excellent test of the inductive force.

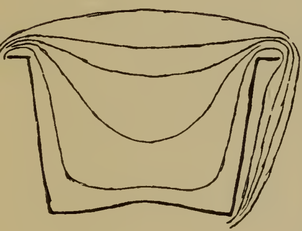
2716. The copper was charged by a Leyden jar up to about 45° of Henly's Electrometer. The effects produced were very beautiful opposite different parts, and gave the same general result as before, i.e. *that where the copper was least charged as shewn by the contact ball, there the induction was strongest; and vice versa.*



2717. Thus the distance at which sound began was least at *a*; it was more at *b*; still more at *c*; more still at the inside about *i*; more (and about the same with each other) at *e* and *f*; but most at *g*.

2718. In the inside of the boiler the effects are influenced by the whole of the surface (as they are indeed on the outside by all that is in sight). Thus the induction at *h* was great, for the whole surface acted on it at once; and if the end of the wire were carried horizontally from *h* towards *i* or *k*, though much nearer the metal, the inductive effect was not much increased; and if at a higher level, as *l*, *m*, etc., there was even diminution as the metal approached. Of course there is a great conjoined action of all the parts on the wire at *h*, *l*, etc., or within the vessel, and thus a complicated result.

2719. This a very beautiful condition of induction, as to increase of each part in action on the force at the wire at *h*, etc.; and also as related in electrolytic induction to the wire of zinc in the middle of a globe of copper filled with acid. Involves probably true explanation of Daniell's large copper surfaces, etc.



2720. The above sketch[1] represents generally some of the planes of equal intensity determined by the wire. The differences may be obtained either by using the same wire, with the boiler more or less electrified; or by using wires terminating with different degrees of acuteness, the copper being charged to the same degree.

2721. The copper was then *negatively electrified* by a Leyden Jar and Henly's Electrometer and the examination made with the wire as before. The sound occurred suddenly as before; the light appeared now as a brush.

[1] In margin.

2722. The relative intensities by inductive action were just the same as before with P. Electricity.

2723. But the jar seemed to be discharged sooner and the effect to fall more rapidly, for which reason proceeded to warm the jar, insulating stool, electrometer stand, etc.; then repeated the experiment and did not observe any particular rapidity of discharge.

2724. The relative effects with the wire were as before.

2725. The relative effects with the carrying ball (   ) were also correspondant.

2726. When the large 10 inch ball was hung in the inside of the charged copper, the latter being in connexion outside with a gold leaf electrometer and electrified, there was no change in the divergence of the leaves; but when the ball was touched by an uninsulated wire, a spark passed to the latter and the copper electrometer fell. On taking out the suspended ball the copper electrometer rose to its first height, and then the ball could give another spark to the wire or the hand.

2727. This is in fact common induction—again it is the Electrophorus—the condenser—and the Leyden Jar.

2728. An insulated needle, lowered into the copper, did nothing unless it were long and so extended into the upper air. Wires and long conductors act as if the electricities at their extremities, or the matters there, were in contact.

2729. If an uninsulated point of a wire were brought near the upper end of the suspended needle, then the former helped the latter—but not much if it were short.

2730. An important consequence and fact that *induction* is best when a point is opposed to a larger surface, and not when opposed to another point. This of great consequence when followed out.

2731. Must endeavour to know how the electricity really passes when the electricity at rough point breaks into streams. Is it that air actually electrified, and that each electrified particle finds its way by itself to the next uninsulated or contrary surface?

2732. A delicate pair of gold leaves were put into a very thick glass vessel (at least half an inch in thickness) and this put into the bottom of the copper. It did not diverge in the least—being of course connected with the copper by a wire. Thus thick glass as the electric acts the same as thin glass or as air. On bringing a

ball to act by induction on the outside of the glass about *a*, it did not affect the gold leaves until electricity was, with the cracking sound, transferred to the surface of the glass there, and then the leaves were attracted on the inside. This must have been an action through the glass, but probably mixed with conduction on its surface, etc. etc.

2733. The approached ball did not act on the gold leaves so soon as it would have done through air alone, I think; but then probable that because of dense nature of the glass, the inductive force cannot penetrate or polarize so deeply and that therefore induction was rather through *the air* towards the bottom of the boiler.

2734. This expt. suggest[s] several other relative to the comparitive penetration of induction into different electrics.

2735. A conductor in two portions put endways into the electrified copper, and then separated, removed and examined. The effect the same as with insulated conductors under the common form of induction, i.e. part *a* rendered Negative and part *b* positive.

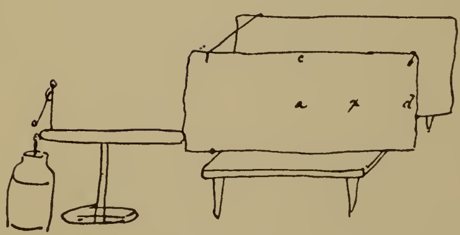
2736. When a large ball, uninsulated, brought opposite the plane surfaces of the inside or outside of the boiler, the latter being in connection with the conductor of the machine, the sparks were of about equal length in both places or parts; near the edges or angles outside they were somewhat longer.

2737*. Two copper plates, each about 19 × 46 inches, were set up edgeways on the insulating stool, about 2 feet apart, and connected by a wire above. They were charged positive by a jar to about 45° of Henly's electrometer—and then examined by the $1\frac{1}{2}$ inch carrying ball. On the outside there was least at *a*; most at *b*; intermediate at *c* and *d*. On the inside there was the least opposite *a*, scarcely a trace; there was more about *x*, and more still, a considerable proportion, towards the edges of the plates, as inside of *c* or *d*, or better still *b*.

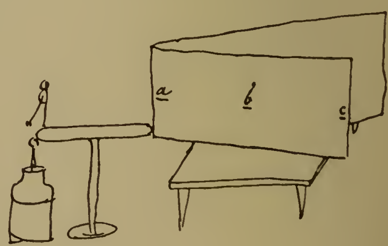
2738. When the inductive effect on the wire end (   ) was obtained the results were just the reverse, i.e. the inductive effects were least where the contact effect was the greatest, and greatest where contact effect least.

2739. The two plates of copper were then arranged anglewise—thus†. No electricity was found by the carrying ball at the inside

* [2737]



† [2739]



near *a*; at *b* little or none; at *c* plenty. The outside was as before.

2740. On using the wire for induction, the effect was weakest at the outside corners and edges, and strongest at the inside angle and surfaces (broad)—in accordance with the former results.

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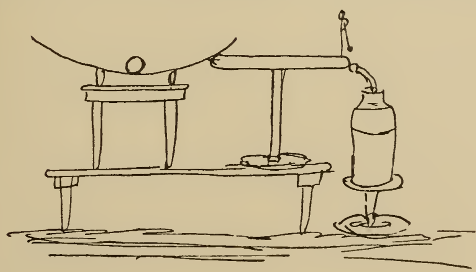
2741. It may be a question whether every case of electric residence on the surface of conductors in our atmosphere is not a case of induction, or whether electricity can reside on such surfaces without induction, solely by virtue of that capability which the air may have to receive a state of polar tension to a certain depth. If the first case were true, then a metallic surface exposed towards space, as a cloudless sky, and having no terrestrial or conducting body in view, ought to have no electricity; whilst if the latter were the case, it would have a certain tension.

2742*. To illustrate this point and its consequences, a tinned copper mirror 24 inches in diameter, and a radius of about 15 inches (its versed sine being therefore about 6 inches), was with the attached cylindrical conductor and Henly's electrometer insulated on a stool; and then a charged jar brought in contact with it, as on former occasions. When this was arranged on the Lecture room table, the mirror of course then being towards the ceiling and distant induction being therefore allowed (i.e. if it could have any influence so far off), then the 1½ inch carrying ball could bring no electricity away from the center of the inside of the mirror. Nor even the 3 inch carrier, that part, even with that ball there, being masked by the effect of the surrounding edges and parts. But when the 3 inch ball was left there to elevate that part, and the 1½ inch carrier was brought into contact with the top of it, then a little electricity could be obtained. This therefore was the limit of the commencement of the appearance of electricity at that middle part—the distant inductive action of the ceiling, etc. being allowed.

2743. Then removed all this apparatus to the roof of the house, into a situation where a straight line drawn from the upper point of the 3 inch ball in the mirror, and passing by the edges of the mirror, could reach no external object except the starry sky. The

* [2742]



sky was clear; the stars bright; the cold great, being about 27° or 28° and freezing fast at the time.

2744. Then repeating the experiments as within the house as nearly as possible, the same electrical effects were produced. Hence electricity can exist upon surfaces which are not inductively related to other surfaces of conducting matter.

2745. Hence an apparent limit to the extent of the action of these electrified surfaces into the surrounding electric.

2746. Hence also pretty sure, that if a very large metallic vessel could be insulated and electrified, that electricity would appear upon its inner surface; i.e. that at last the distance between the opposite surface[s], etc. would be so great that their reaction would be insensible.

2747. Perhaps can obtain this effect with smaller vessels if other electrics than air can be used—probably make this out when the ratio of the electrics is better made out.

2748. I did not find any sensible difference in the electricity of the carrying ball when I allowed my hand or head to come into sight line with the inside of the mirror, provided they were at 18 or 24 inches distance.

2749. Conclude therefore that a plane metallic plate, having one face towards the clear heavenly space, would still shew signs of electricity. This a normal quantity in any given electric for any given intensity.

2750. Repeated the experiments in the house again and had the same results as before.

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2751. In a boiler the inner quelling effect ought to cease probably when the vessel is so large that the inductive action at the center ceases also; probably that would be the limit.

2752. Would not a radius of 12 feet do for this—or at least allow of some appearance inside?

2753. Perhaps worth while to ascertain corresponding size and distance of two parallel round plates when electricity just lost at center of the inside.

2754. Would these be the same for different charges or vary with the intensity of the charge?

2755. Must charge metals, liquids, etc. in very thick glass vessels, avoiding induction, and then see their effect on the electrometer.
2756. Or charge electrometer and then touch it with a mass of metal or fluid in such a glass vessel, and see how it is lowered. Compare also the lowering effect by a vessel full of the matter to be tried, or its sides lined with it only.
2757. Every thing seems to indicate that there is extremely little, if any, electricity in the mass of metals or conductors, and hardly any even in electrics or such bodies, except in their state of tension or induction.
2758. It appears to me at present that *ordinary* and *electrolytic* induction are identical in their first nature, but that the latter is followed by an effect which cannot, not from the nature and state of the substances, take place with the former.
2759. In connecting these two actions, interpose decomposition of water in close tubes. Also Davy and Herschell's motions of mercury in solutions. These conjoin the cases of the attractions of *masses and particles*.
2760. Metal leaves or other conducting masses which shew attractions in air, are exactly in the same relation as metals in the voltaic circuit—they act merely as conductors in both cases, allowing the surrounding electric or electrolytic matter to assume certain states.
2761. Hence the attractions of masses are resolvable into the attractions of particles, and a direct consequence of the latter. Here again the motions of mercury come in as connecting links.
2762. To prove *identity of electricities* (Series iii) is to prove *identity of their effects*.
2763. As to state of tension in the electric or electrolyte before separation of elements. Try some body not yet decomposed and not a conductor between the poles or conductors of E. Machine; and observe action on light.
2764. Try induction through a solid crystalline body as to the consequent action on polarized light.

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2765. Is the P. and N. light the same in character in different gases and vapours, i.e. in anïons and catïons? May perhaps obtain

a relation to their inherent electrical state. Is possible that their intimate nature may produce differences. Try Oxygen, Hydrogen, etc.

2766. Can induction through air take place in curves or round a corner? Can probably be proved experimentally. If so, is not a radiating action, and reasoning as to sky action requires modifying.

2767. Consider effect of rarefaction in causing discharge. This appears to be very simple if the charged state relates to the charging of the particles of Electric. The force of definite action come in here.

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2768. Electricity appears to exist only in *polarity*, as in air, glass, electrolytes, etc. Now metals, being conductors, cannot take up that polar state of their own power, or rather retain it, and hence probably cannot contain developed electrical forces.

2769. Metals however probably hold it for a moment as other things do for a longer time; an end coming at last to all. See Wheatstone's experiment shewing a retardation, for the middle spark was late. Now metal in this may be the extreme case one way as air, glass or shell lac are extreme cases the other way.

2770. Electricity then is probably no accumulation of power *en masse* IN ANY CASE, but only an arrangement of particles in polar position.

2771. But must in that case make out the true relation of discharges, brushes, sparks, etc.; which seem to be actual transfers of force. At the same time, these may be resoluble into such cases as the discharge by particles of electrolytes; only taking place violently through many particles at once, and hence the heat. Then they will still fall into the general law.

2772. In investigating the distinct electrical phenomena of conduction, induction, transfer, etc., hesitate not to examine them in coarse or strongly developed cases, for the sake of gaining information in the weaker. Thus the induction which can take place between the ends of a powerful Voltaic battery in air, glass, or water is the same as what occurs between the *same ends* in an electrolyte, *solid or liquid*, or as that between weaker ends or even

a single pair of plates. The spark between two metallic surfaces, being either the ends of a voltaic battery or of the coatings of a Leyden jar, is the same as that of a single pair of plates on junction or disjunction. The conducting power of metal is the same as the conducting power of air. The insulating power of air is the same as that of the retarding power of metals.

2773. It is important to make out the true nature and condition of charged air or metal. Is it possible to add in the least degree to the actual quantity of electricity in matter? Many things seem to shew that it is—thus, brushes into air; Wheatstone's retarded spark; are the two halves of the metal for the moment charged above their natural state?

2774. Is not the *time* occupied between the extreme and the middle sparks in Wheatstone's experiment the same element in the phenomenon as the time occupied in charging the air by brushes or a point; and the discharge of the same air against the sides of the room, an electrometer, etc.

2775. Does it not appear in many cases that when air is charged, it can be discharged without every particle coming in succession against a conductor. It has often seemed to me as if the electricity could proceed from particle to particle, and that with great quickness.

2776. Consider here the mode in which it passes from surface of inside of a charged boiler to a point.

2777. Can probably arrange this so that, as air should be always issuing from or rather passing to a point or flame, still it might be made to receive a charge; and again, so that the air really should not change at point whilst receiving, etc. etc.

2778. As glass can be excited and so act as a prime electric in producing induction in air, why may not air itself be also excited (i.e. charged) and act as a prime electric also, even in air? In that case will be acting by induction on the surrounding matter.

2779. Must consider closely the circumstances of electricity fixed as it were and localized on the surface of a *solid electric*—or on conductor; and that given to the particles of a fluid mass, as air, oil, etc.

2780. The disturbances and excitations of the particles of bodies in mass are probably only minute effects of the same kind as those

which take place in a vastly higher degree in the particles of bodies suffering chemical changes; as for instance the change of particles of oxygen and hydrogen before and after combination; the change of lime and carbonic acid when combined or separated by the action of heat—of oxygen in and separating from chlorate of potassa, etc. etc. etc.

2781. The excitation of a glass rod by friction may be due to an approach in the slightest degree to chemical action between the particles brought by rubbing into contact with each other, and then left by lateral motion of parts in this excited state; the excited state being an infinitesimal portion of what chemical action would or could produce.

2782. It is essential to know whether a particle of matter can be *overcharged or not*; but very difficult to decide. The excitation of glass, resin, silk and other solid electrics, without the use of conductors, seems as if that could be the case.

2783. With relation to the condition of conductors relative to electrics in contact with them, must try the power of a certain ring of metal, as *tinfoil*, by itself; i.e. in air—on glass solid—within glass—on thin glass, as a phial—with oil inside—water inside—metal inside—immersed in oil—in wax, etc.

2784. Perhaps a globe or a plate will do for these, or at least the immersion experiments; but must take especial care that all are equally free from inductive effects.

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2785. Put up two paper frames, and with them repeated the experiments made on the instant ( ) with the copper plates. They did perfectly well, becoming electrified all over; shewing the same relation by the carrying ball inside and outside either by contact or induction.

2786. May now make a very large paper box, 12 feet square for instance, and repeat experiments with it as to state of inside, center—an electrometer within, etc. etc.

2787. With reference to the possibility that some metals, not magnetic at common temperatures (2613), might become so at low temperatures, selected pieces of the following metals and ascertained that they did not, by close approximat[ion], affect any part of an astatic needle (or one nearly so). This kind of needle was used as the most delicate test.

Lead	Bismuth
Copper	Antimony
Gold	Arsenic
Silver	
Platina	
Palladium	
Tin	
Cadmium	
Zinc	
Plumbago	

2788. Those in the first column were in wires, except the plumbago, that being a small rod—about $\frac{1}{30}$ of inch in diameter and 0·4 of an inch long. They were put altogether and tied round with a bit of fine platina wire so as to form a small faggot, a piece of platina wire about 3 inches long being tied up with them so as to form a handle of badly conducting metal.

2789. The other three, Antimony, Arsenic and Bismuth, were tied up in a similar way in a piece of platina foil to form another bundle.

2790. Then cooled these bundles in liquid sulphurous acid, in watch glasses containing a little mercury also, and when below the freezing point of the mercury, brought them close to the ends of the astatic needle to ascertain if they had become sensibly magnetic; but could not observe the least indication of such an effect, though I think the temperature must occasionally have been 60° or 70° below Zero of Fahrenheit.

2791. Brought the frozen mercury near the needle, but still there was no effect.

2792. These experiments were made on the top of the Lecture room; the astatic needle being also out of doors. The temperature of the air was about 25° F.

2793. Now experimented with Nickel, Cobalt and Chrome as to the points at which they and Iron lose magnetic relations.

2794. *Iron* ceases to be magnetic at a fair or rather bright red heat.

2795. *Nickel*, I found, ceased to be magnetic at temperatures very much lower. The nickel was such as had been purified by solution of ammonia. It was a bar (forged) nearly 2 inches long, 0·4 of inch wide and about $\frac{1}{15}$ of inch thick. At temperatures below visible red, it was not magnetic. In boiling Almond oil it was not magnetic, but at a temperature a little lower became (like iron) suddenly magnetic, and apparently to its full power at once. In oil hot enough to boil mercury in a closed thermometer it retained its magnetic power, and this was about 612°; but when probably not more than 20° hotter, it had lost magnetic power. Hence about 630° or 640° is the temperature at which Nickel looses or regains magnetic inductive force.

2796. *Cobalt* has very little magnetic power at 50°; in fact, I could not by the present needle render it sensible. Being cooled by Sulphurous acid to at least 50° below 0° of Fah., it was still the same—not at all Magnetic.

2797. *Chromium*: a piece that Cooper gave me a long while ago, and very spongy; not fuzed. It was not sensibly magnetic either at 50° or at 50° below 0° F.

2798. I doubt the magnetic property of these two metals.

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2799. As to the nature of the light at P. and N. points. It may have reference to the nature of the substance of which the points are made. Now these are almost always metals or catïons, i.e. bodies inherently positive. Must look at them with this in view; it may cause the difference at P. and N.

2800. Try points of different metals with high and low equivalent numbers—opposed to each other, etc.

2801. Take an anïon if possible for P. and N. points.

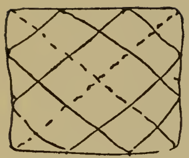
2802. Try oxides of iron—per ox. manganese—sulphurets of antimony—iron, etc. etc. Plumbago—boxwood charcoal, etc.

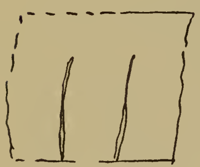
2803. Metal points greased or covered with wax.

2804. Try various points in atmospheres of anïons and catïons. Acids, alkali, Hydrocarbons, etc. etc. etc.

2805. Try them in liquids, oils, etc., in pure water. See 4 July 1836.
2806. Points rubbed over with iodine—points of iodine.
2807. It has seemed many times as if, when platina wire is in circuit, that the heat has risen first on the Positive side and also highest—verify this.

2808. Have been for some days past engaged in building up a cube of 12 feet in the side. It consists of a slight wooden frame, constituting the twelve linear edges, held steady by diagonal ties of cord; the whole being mounted on four glass feet, $5\frac{1}{2}$ inches long, to insulate it. The sides, top and bottom are covered in with paper (   ). The top and bottom have each a cross framing or tying of copper wire, thus: which, with the diagonals of cord, support the two large sheets of paper which cover them in, the copper wire also serving to feed the paper surface with electricity. The framings at the top and bottom, of copper wire, are connected by copper wires passing down the four corner uprights; and a band of wire also runs round the lower edge of the cube. The sheets of paper which constitute the four sides have each two slips of tin foil pasted on their inner surface, running up $\frac{3}{4}$ of the height; and these are connected below with the copper wire so that all the metallic parts are in communication. The edges of the side sheets are fastened here and there by tacks or paste to the wooden frame at the angles, so as to prevent them flying out and so giving irregular dispersion of the electricity. The whole stands in the Lecture room, one of the lower edges being within 5 inches of the third seat (on which the feet rest), and the opposite lower edge being sustained on stools and blocks, about 4 feet from the ground. The chandelier hangs nearly opposite the middle of the face of the cube at this side, being about $2\frac{1}{2}$ feet from it.





2809. The cube rises in the middle of the room above the level of the bottom of the gallery, and appears of enormous size; it holds of course 1728 cubic feet of air or about   lbs. avoirdupoise.

2810. Connecting this cube by a wire with the Electrical machine, I can quickly and well electrify the whole, so as to make a Henly Electrometer stand at 70°; but then the dispersion at angles, edges, etc. is as great as the supply, and when the machine stops working the electrometer quickly (though gradually) falls. Whilst the machine continues at work the hand attracts and draws the paper

towards it, and brushes can be drawn from every part. Sparks can not be obtained except where the wire feeders or wood work passes, because of bad conducting power of the paper; still, it is fully charged and sufficient for my purpose in most respects.

2811. If a conductor and Henly's electrometer be applied against the paper only, the instrument open[s] as much as another electrometer at the machine, shewing full charge for the time. But again, if the hand be applied in contact with the paper at a part equidistant from the different leaders, the discharge there does not cause the electrometers elsewhere to fall much, provided the machine continues at work. Hence a proof of the bad conducting power of the paper, and also that by vicinity and contact of points, knobs, cushions, etc. to different degrees in different parts, the portions of the cube may be very differently charged at the same time; the leaders, of course, always being as highly charged as the machine, whilst the loose edges of the paper at a distance from the leader will be lower in degree, as always giving off.

2812. Proceeded to experiment with this cube, as follows. The large brass carrying ball (   ) was used, being suspended by a white silk thread to the end of a long glass rod (flint), in order to avoid as much as might be the inductive effect due to opposing the hand and body to the part from where a sample was to be taken. The machine and cube were retained connected by a wire. Three turns of the machine were given (one and a half bringing the Henly electrometer up to its full degree), whilst the carrier ball was held in contact with the part to be tested. The ball was removed from contact before the machine stopped—the conductor and cube discharged and the ball carried to the delicate electrometer (retained in a sheltered safe place so as not to be affected either by the machine or cube), and examined as to its charge and the degree acquired.

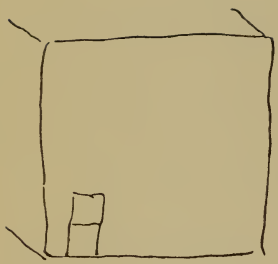
2813. In this way, results exactly like those with the boiler were obtained. A small charge was found on the middle of a face, as clear as possible under the circumstances from inductive influence. A stronger charge, by much, was obtained at the middle of an edge; and the strongest at the solid angle or corner.

2814. Those parts where induction was active, as opposite the chandelier, or opposite the seats, etc., gave an increased charge

(   ). The lower part all round therefore gave more than the upper, and there was a greater escape of electricity there.

2815. By testing for the results when the carrier ball was the extremity of the inducing body (   ), the same general results as with the boiler were obtained, and such was the case also by using the wire (   ); but the necessity of keeping up the charge of the cube to the overflow and dispersion of electricity at the angles and corners interfered a little with the results.

2816. Now experimented by bringing the carrier ball in contact with the inside of the cube, taking always great care to lift the ball from contact before the machine stopped working, and also to discharge both machine and cube before the ball was brought out into the air.

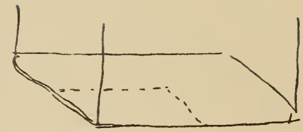


2817. Access to the inside was made by cutting a flap in the paper at the side nearest the[1] Lecture table towards the left hand corner and pinning it up for the time. The apperture was 14 inches from the left hand side, 22 inches wide and 24 inches high.

[1] In the manuscript, folio volume II finishes at this point. The following words of par. 2817 are from the first page of folio volume III.

FOLIO VOLUME III
OF MANUSCRIPT

2818. The bottom surface inside was now examined as far as to the middle, that being as far as I could reach with the ball and glass rod; but no electricity could be obtained there except near the door or entrance, the reasons of which will appear immediately. Now this portion is the same with the similar portions of every other part and in that respect includes the whole of the inner surface of the cube. Hence when fairly and properly charged there was no electricity inside this great cube.



2819. The same trials were carried up the left hand inner surface as far as I could properly reach, with the same result, and also up the face within nearest to me, where the same results occurred with the exceptions which, with their causes, will be described immediately.

2820. On making the inductive experiments on the inside, the same results were obtained as before. If whilst the ball was in contact 3 or 4 feet within the cube the hand was put an inch or two through the opening or door way of the cube and kept there until the ball was raised, and then the cube discharged and the carrier examined, it was found Positive; and that the more so the nearer the hand was to it and over it. A wire point produced an effect even when several inches outside the opening. Looking into the house, even though the head was preserved at some distance so as not to discharge more electricity than could be helped, had the same effect.

2821. It was this effect of induction which in several cases caused the appearance of Positive electricity on the inner surface near the opening into the cube.

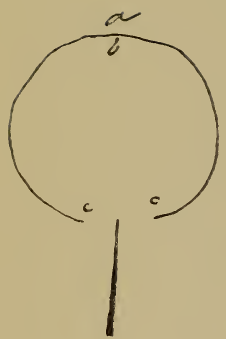
2822. When the other class of inductive effects were observed they also corresponded with former observation. A ball held in the cube in the air and touched by a conductor became highly Negative wherever held, and apparently more so in the corners and angles than in the more open space.

2823. A wire was put through a long glass tube and the end allowed to project a little, the other end being held in the hand. The first end of this wire being put into the cube became highly

negative, and luminous. The sound of discharge I could not hear because of the noise of the working machine. But the star of light was brighter when nearer to the sides than at the middle, and at the same distance from the side was brighter if brought near to the inner linear angle, and at the same distance from that was brighter if brought towards a corner angle, so as to have effect of vicinity of all sides at once.

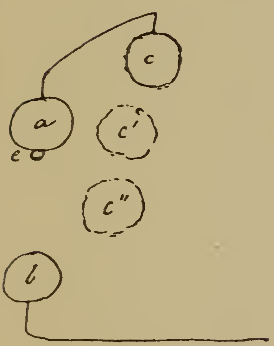
2824. But for reasons connected with the discharge of the paper surface and its slowness of recharge because of bad conducting power, the experiment to be accurate should be made in a metal vessel. As indeed it was in the boiler (   ).

2825. I very much incline to believe that inside a metal globe the effect upon a point or upon a carrier ball would be the same in amount wherever that ball or point was placed, either in the centre or near the side. Well to prove this as a fundamental principle of the action of induction.



2826. It appears to me that induction is illimitable, i.e. that a point opposite to a charged surface and rendering the surface more highly P. or N. will do the same thing to the same amount whether it is engaged in violent inductive action elsewhere or not. Thus if the globe *a* be Pos. and a point at the aperture, that point would render the inner surface at *b* as much Pos. to a carrying ball if it were receiving a strong electrification and even discharge by induction from *c*, *c* as if it were not.

2827. And yet this can hardly be true, for consider the case of a point in a ball, etc. etc.



2828. Suppose *a* and *c* two metal balls in metallic communication but insulated so that *c* can be moved, and both charged Pos. by a Leyden battery so as to retain them up to a full charge; *e* is a small carrier ball, and *b* a fixed ball connected with the earth and acting by induction on *a*. In the position *a*, *b*, *c*, the carrier ball *e* will convey to the electrometer a certain charge. If *c* be brought to *c*′, it and *b* will have a stronger inductive reaction, but will that affect the relation of *b* and *a*, i.e. will *e* carry a less charge to the electrometer. Or if *c*′ be brought to *c*″, when *b* will be still more highly affected, will that affect its relation to *a*.

2829. Unless *a* and *c* have an unlimited supply of electricity, of course the change in *c* will produce a change in *a*; *b* may be a

point and that would be the best mode of obtaining the result perhaps. Must also consider the mutual action of *a* and *c''* and remember that, without *b*, moving *c* to *c''* would effect the state of the carrier ball at *e*. This proportion of effect must be allowed for. Perhaps best done by keeping *a*, *b* and *c''* fixed and constant and then occasionaly introducing a thick plate of glass or several plates between *b* and *c''*, so as to prevent and renew induction at intervals. *e* ought to give constant results.

2830. To return to the cube of paper and its state within. There is another condition and cause of parts of the inside which results in their being electric. Thus near the door and elsewhere parts were found Positive inside, which could not be accounted for by any effect of induction from objects without.

2831. This was traced to inequalities in the discharge of different places or parts without; thus when the upright edge was loose at the side to the left, on electrification it swelled out from the wooden upright and, discharging electricity into the room, was reduced in its electricity and though supplied by the nearest leader, still because of the resistance of paper to conduction, was not so high as that leader. Such a part was therefore in a deficient state as compared to the rest both on its inside and outside surface; induction therefore could take place between the inner surface of that part and the inner surface of another part fully charged. The latter therefore shewed signs of Positive electricity by contact with the carrier ball, and the part in relation to it by induction within gave upon examination the *Negative state* to the carrier ball.

2832. Once knowing the cause, the effect was easily followed out. Thus on applying the hand against the outside of the cube at that part of the paper clearest from leaders of Electricity, the inside was found by the carrying ball strongly Negative and all the neighbourhood within in a positive state. The negative and Positive within were probably the exact equivalents of each other. This was therefore a case of internal induction and not of an independant pos. charge, and could not have happened in a metallic vessel.

2833. Then placed a discharging point, i.e. a point connected with the earth, opposite to and about 18 inches from the outside of the cube at a part of the paper furthest from the leaders and

electrified the cube. During the whole time the point was luminous and receiving electricity from the cube by inductive transmission. The point was Negative and the opposed paper surface of course Positive from the machine. But the inside of the cube at that part was strongly Negative and the neighbouring parts a good way round within Positive.

2834. Where external edges, or the vicinity of external objects as the chandelier, the seats, etc. caused discharge of the neighbouring parts so as to lower their state below that of the feeders or other parts, then internal induction took place, as might be expected, and it was impossible to obviate the effects of this entirely in so large a cube, included in so small a space as the lecture room, and built up of such a material as paper with metallic wires and leaders here and there only. With a continuous metallic vessel such effects ought not and would not occur.

2835. But whenever it occurred it could be traced, for the *two states* were sure to be found within and as far as I could judge in equivalent quantities. Hence so far the large cube does not acquire a general charge within though so much air included.

2836. It is very curious thus to see how high a negative state may be induced and obtained within the cube, the *whole of which* is originally and continuedly in a Positive condition by contact with the machine.

2837. Arranged a table under the cube; its upper surface was about 6 inches from the bottom of the cube; put on the table an insulating stool (feet 6 inches long) which therefore served as a platform in the cube and yet preserved the insulation, and now experimented upon the state of things within.

2838. Placed a delicate Gold leaf Electrometer within the cube, closed the flap of the door of the cube and removed all external objects as far as could be done to prevent them effecting discharge from the outside and so producing internal induction. Looked at the Electrometer through a distant slit. Then on electrifying the cube even so highly as to obtain strong sparks from the corners and leaders, the Henly electrometer on the machine being at 90°, still there was no divergence of the gold leaves within. Whether the Electrometer was placed in the middle of the cube or towards the side or in the corners, still no deflection. If the

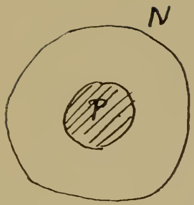
cube was charged suddenly or even discharged suddenly, still no deflection or divergence. In fact, no matter the state or disturbance of the electricity of the cube, provided no internal induction was allowed, the gold leaves were perfectly indifferent to all the rest.

2839. On opening the cube door, then wherever the electrometer was placed within, the introduction of the hand, the head, or a wire made the distant leaves diverge positive. When the electrometer was in the middle of the bottom so as to be open to view from without the door, then approaching the hand or a wire to the door and before either were in, made the leaves diverge. These of course were cases of induction from without and illustrated the position before laid down, that where electricity by contact is weakest there that by induction is strongest.

2840. Leaving the electrometer inside and closing all up, if a wire or other discharging agent were put opposite the outside away from the leaders, then the electrometer opened within. This is a case of internal induction (   ). By moving the wire opposite the face of the cube, it was very beautiful to observe how the gold leaves fell again as the wire came opposite the different leaders and how as it passed opposite the paper surfaces away from the leaders they rose again; also how on removing the wire altogether the leaves fell as the parts discharged by the wire rose up to an equal state of tension with the rest.

2841. Must remember that these inner effects only occur because of bad conducting power of paper and not because of any necessary relation of inner and outer surfaces. If the vessel had been metallic, no drawing off of electricity or elevation of its intensity there would have in the least affected the inside. But as the outside is discharged, at that part, and actually lowered, the thickness of the paper being too small to allow enough to flow in laterally from the leaders; the inside is relieved from the tension which it ought to have to oppose the other inner parts and they therefore produce an evident induction action towards it. The action was there before, but was equal in all direction[s] and null as to P. and N. compared with the natural standard.

2842. But then, what is natural standard and what is Pos., what is Neg.? (–the inside of a metallic vessel). There is no doubt that mites living on the outside of a highly charged globe in air but



free from the induction of extraneous bodies or else subject only to a constant regular induction, could not tell that by any effects amongst themselves, and that all their electrometers and machine[s], etc. would have the same relations, etc. on the highly charged surface of P as ours; further that other mites living on the inner highly negative surface of N would be in the same condition amongst themselves; that the laws of action discovered on the inner surface of N would be the same as those applying at the outer surface of P, Voltaic action and all included. What then is Pos. and Neg., and what especially is *inherent positive and negative states*.

2843. *Bodily charge of the air*. Endeavoured now to obtain some proof of the bodily charge of the air in this cube.

2844. First continued the cube charged for some time, the Electrometer being within. As already said, the leaves did not at all open. Then discharged the cube and watched to see if leaves would then open, from communication of air electricity to them, but nothing of the kind took place.

2845. Charged the whole, then discharged the cube and touched the cap of electrometer for a moment with a wire—still no effects.

2846. Charged the whole, discharged the cube and held a ball connected with the earth near the cap of the electrometer—still no effect.

2847. Hence no apparent charge of the air within the cube.

2848. Then electrified the cube itself through the air within, thus. Fixed a glass tube above 5 feet long into the side of the cube pointing directly inwards. A wire of copper was passed through the tube, one end being made fast to the machine and the other projecting towards the middle of the space in the cube. Worked the machine and soon found the cube fully charged so as to give excellent sparks and brushes, and all this had gone through the air of the cube. But still the electrometer was not affected and on suddenly discharging the cube and machine, and then watching the electrometer to observe whether the air would charge it, it did not diverge in the least.

2849. Hence no bodily charge of the air this way.

2850. In like manner the glass of the electrometer took no charge.

2851. If, whilst the cube was charged, a wire point connected

with the earth was introduced into the cube, the electrometer as before said opened ( ), and if the point were held near the cap, the leaves took a slight rapid quivering motion; and if after a time the rod or wire were removed and the cube discharged, and the electrometer also touched so as to collapse, it gradually opened again. If it were again collapsed by a touch and again left, it again opened and so on three or four times. But this was due to a charge of the two surface[s] of the glass, for the end of the wire was N., the top of the cap P., the lower ends of the leaves N. and the general bottom of the cube inside P. Hence the two surface[s] of the electrometer jar could be easily charged; that they were charged was easily shewn by repeating the experiment and immediately taking the electrometer *out of the cube*, when the successive divergences took place, as before. Hence the air was not here charged and the cause of error if it occurs may be avoided or detected.

2852. I now went inside the cube standing on the stool and Anderson worked the machine until the cube was fully charged and he continued working the machine. I could by no appearance find any traces of electricity in myself or the surrounding objects. I could not affect the gold leaf electrometer within. But if I brushed it with flannel it was excited in the usual way.

2853. A glass rod was excited by silk and amalgam and gave the usual appearances, either to myself or the sides of the cube, and had its usual effect upon an electrometer within.

2854. A point had no action on things within, unless I produced extra excitation within, and then acted in the usual manner.

2855. A candle burnt within without shewing any effects on the flame—or the flame affecting the electrometer or shewing any thing it would not have shewn in the open unexcited air: it was quite indifferent.

2856. A leaf of gold held at the end of a wire shewed no attraction or motion except what due here and there to effects of internal induction before pointed out ( ) and these were very feeble.

2857. A leaf of gold was driven about in the air by an excited glass rod, exactly as in a common room unelectrified.

2858. In fact the electrification without produced no consequent effects within, other than what belong to any open chamber.

2859. But if any thing from without connected with the earth was introduced into the cube through the door way, then the flame, the gold leaf, the electrometer, the hand and all these things were very strongly affected in relation to it, they being Positive and it Negative.

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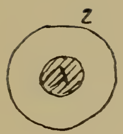
2860. Consider the difference of solid and fluid insulators. There is a kind of conduction or discharge in the latter probably which the former [do] not allow of. It may be considered as a slow discharge, for a point can not discharge a jar *at once.*

2861. Probable that the intensity is a constant quantity for discharge through air, whatever the shape of the part, whether ball or point. But must consider depth of air, etc. and other things.

2862. Is there not then a direct analogy between a rounded surface in common electricity and amalgamated zinc in voltaic electricity?

2863. A metal point in flat glass or other fixed electric; why is it protected?



2864. If 1 be a metal globe placed within a second metal globe 2, and then 1 be electrified Positively, then as much P. Elect. as was given to the surface of 1 ought also to appear on the surface of 2—the inner surface of 2 being equivalently negative. For all expts. shew that there is no waste of power.

2865. If there were a thousand such concentric globes, the inner one only electrically charged, *all* the others ought to be equally and fully charged; for metal takes *no electricity* and the air electricity is *all polar.*

2866. *As to induction in curved lines*, etc. Removed the insulating stool from beneath the cube and placed a point connected with the floor under the middle of a part of the paper squared in by the lines of wire work. Then electrified the cube and by the carrying ball examined different parts of the inner bottom surface. That over the outer point was negative. All round it the surface was positive. Thus this case of internal induction is all in one plane and shews that the surfaces need not be opposed to or in sight of each other.

2867. Induction therefore may doubtless take place in curved lines. It does so in decomposing solutions, as is easily shewn by

its turning a corner, and that it does so in air is easily shewn also as above and by the spark passing round the edge of glass, or by a point discharging a surface from behind a glass, etc. etc.

2868. The table which was 6 inches from bottom produced similar effects. Opposite the edges and the nearer parts a more abundant discharge took place than elsewhere and there the inside was Neg., the middle part and the surrounding parts being Pos.

2869. The electrometer inside, if leaves connected with leaders and discharge outside avoided was not much affected; but if connected careless by longish wire going to parts more or less easily discharged without, then it was affected by internal inductions.

2870. The electrometer was put inside and then a point placed opposite a part of the paper surface near on the outside—it was partly discharged and a case of internal induction occurred and the gold leaves were affected and attracted towards the inside. On removing the point and holding a globe of brass in its place and even much nearer still the Electrometer was not affected. Hence former statement proved (   ), for affecting the intensity outside did nothing within, though withdrawing the electricity well might. The ball in contact did not do so much as the point at a distance.

2871. To shew bad conducting power of the paper. A sheet of tin foil was laid on a neutral portion of the paper inside, another sheet [a]cross the place of a leader and its angle came near the first. A point was placed beneath the neutral portion of paper and the machine worked; sparks continually passed from the leader tin foil to the neutral tin foil, being the surplus of what the point beneath discharged which the *paper could not conduct*. This a sufficient cause for many of the effects.

2872. Even when the point removed, the edge of the table discharged enough to give a series of smaller sparks above.

2873. The two sides of a leaf of gold, or rather of the air against it, may both be made positive or both Negative or Positive and Negative to any degree by induction. There appears to be no mutual or common influence between them.

2874. If a leaf of tin foil were put between two glass plates and made highly positive on both sides by induction, would there, on the removal of the plates, be the least electricity in the foil? No.

2875. Daniell's cobalt called pure is very magnetic. Dissolved a little in N.M. Acid—sulphur separated—to part of the solution added ammonia in excess—per oxide of iron separated—to another part diluted added sol. ferro prus. pot.—prussian blue precipitated. Plenty of Iron to account for Magnetism—think there is nickel also but did not look further.

2876. *Subject.* All bodies to be examined and classed electro-chemically both as Anïons and Catïons, and also in relation to electro-chemical equivalents. There will be probably three, four or more classes, thus.

i. Elements—As anïons and catïons with their numbers.

ii. Binary compounds of these, as oxides, etc.; but what is the state of their force and the number expressing it?

iii. Salts, etc.

iv. Compounds of anïons.

v. Compounds of catïons.

2877. But then what are the relations of these classes to each other, as to their degrees of force, numerically expressed and determined by experiment—and how is this force made out in its actual result in the combinations of chemistry? This a most important subject.

2878. Reason from first class backwards—can there be simpler bodies than Anions and cations—how can they be related to electro chemical forces or what can the state of their particles be? If there be such bodies, then a new electricity or at least a new condition of that power at present unknown to us.

2879. *Subject.* Nascent state—its relation to state of tension before its discharge by current action. Nascent particles are probably undischarged particles having *a tension to combine.*

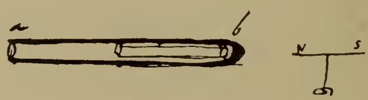
2880. Consider solution of gases in relation to this subject—is it a different state or not?

Daniell's view of adhesion as cause of nascent state and mine also in sixth series (658).

27 JANY. 1836.

2881*. Expt. on the non-interfering power of hot iron, made as follows: N—S was a magnetic need[le] in its natural position, *a b* is an iron tube, being the breech of a gun barrel, open at *a*,

* [2881]



closed at *b*; a magnet about 4 inches long and 0·5 × 0·25 inch thick was also used. Now bringing the Marked end of the magnet up towds. the Marked end of the needle, both being in the same line, the end of the needle was repelled when the magnet was 8 or 10 inches distant. Bringing up the closed end of the cold iron tube in the same manner, there was attraction, etc. on the end of the needle when the iron was about an inch or two off; but no repulsion. Putting the bar magnet into the tube N end first and bringing it up to the needle as before, *there was no repulsion*, but when near, attraction as if there had been no magnet in the iron tube; if approached laterally the tube and magnet was found slightly polar as a whole. Hence the Iron (cold) had masked the effect of the magnet on the needle.

2882. The tube was then made full red hot and had *no effect* on the needle when approached as before. But on reintroducing the bar magnet, the latter acted with its full force upon the needle, repelling its marked end and in fact being just as if air, water or glass only intervened. Hence hot iron had no masking power and was as copper or silver.

2883. A steel magnet when heated loses its polarity long before it loses its power of becoming a magnet by induction. Thus on putting a steel magnet into a tube with almond oil and heating it, the power of its ends to repel the similar poles of a needle disappeared rather suddenly at a temperature nearly the highest which the oil could bear. But at temperatures very much higher than that and up to a red heat, both ends excited strong attractive powers upon either end of the cold magnetic needle.

2884. It appears to me that above the heat of boiling almond oil, steel will receive magnetic arrangement by induction as easily as iron.

2885. A piece of loadstone could not be deprived of its polar power in boiling oil.

2886. Being heated more highly, it lost first its polar power just on the limit of visible ignition in the dark, and at a temperature a little higher or very dull red it lost all power. By varying the temperature above and below this point, it was found to lose and gain its ordinary power like Iron and nickel, but far below the first and above the last. In regaining its ordinary power it did

not regain its original polar power but could easily be magnetised by the needle or a magnet.

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2887. Is evident in volt. battery with its *tension* and its *spark* that *chemical action is electricity.*

2888. Hence also *electricity is chemical action.*

2889. Hence electricity of rubbed glass should be *chemical action.*

2890. Hence chemical tension of *acid and amalgamated zinc* before the current passes must be the same as rubbing glass and amalgam or rubbing wax and flannel *in its origin.*

2891. Induction of battery poles or electrodes in an experimental decomposing cell or in the air with a spark must be the same force. Hence glass electricity and its induction up to the spark must be the same force.

2892. Then charge of air by induction must be a definite action.

2893. As incidental to these principles: What effect ought to arise if the two ends of an insulated battery were enlarged into extensive plates and made alternately to approach and receed? Ought not there to be to and fro currents in the battery? But the *quantity* would be very small.

2894. It is important to make out:

Condition of conductors.
Excitation by friction.
Nature of electricity.
Charge or non-charge bodily of matter.
The back action of combining particles.
The existence or non existence of repulsive force.
The arrangement of substances (2876).

2895. In bringing forward the relation of common and voltaic electricity, must treat a few general points first, as induction—form—residence on electric, etc. and then combine all into one result. Else the proofs and developments will be too brief for the present state of this branch of knowledge.

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2896*. Examined the appearance of the brushes from a positive ball connected with the prime conductor of a good electrical machine in a dark room; the ball was about this size[1], of brass, and clean on the surface; the contiguity of neighbouring bodies was avoided.

2897†. This brush consisted [of] a short conical bright part near the middle of the ball, which at a little distance ramified and passed into pale radii. The rushing sound accompanied the appearance of the brush and by passing the eye quickly across the brush from above downwards, it was found that the brush was not continuous but was the result of successive discharges, for then these could be separated from each other to the eye.

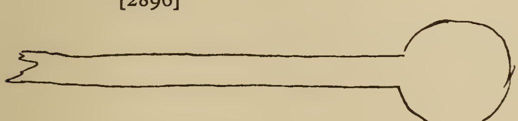
2898. The bright part exists simultaneously with the brush, for each figure of the one has the other. The rushing sound is due to the noise of these discharges and the pitch of the continuous sound can be raised by rapidly working the machine and so increasing the number of discharges in a given time.

2899. On separating the general brush into the elementary ones by moving the eye, the ramifications of the latter were seen more distinctly than those of the former—the former indeed being a confused appearance of many brushes, overlapping each other as to their figures on the retina.

2900‡. On using a smaller ball, the brushes were obtained more easily, that is with less supply of electricity, but they were smaller. Still they were successive and the general brush could be resolved by the eye into the separate discharges—but these were far more numerous than before in the same time. The soft hissing or rushing noise was heard, but more continuous and more feeble than before. The phenomena altogether is the same as with the larger ball.

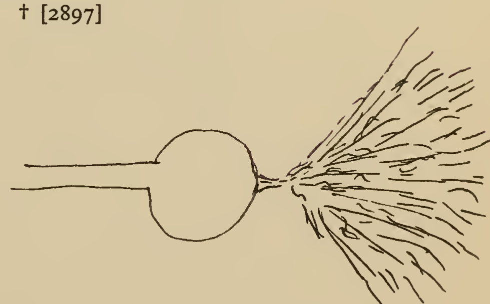
[1] The diagram has been reduced to $\frac{3}{4}$ scale.

* [2896]



‡ [2900]

† [2897]



2901*. Now used a brass wire rounded at the end of about this size[1]. The brushes were obtained still more easily than with the small ball and the size of them, as also of their brighter foot stalk, was diminished. The sound was more uniform and continuous but not so loud.

2902. But now observed a new phenomenon: for on working the machine quickly, when the intensity in the conductor rose to a certain height—the brush disappeared and a phosphorescent glow took its place on the whole of the end of the wire. It did not rise any height from the surface of the wire end into the air nor did it produce a sensible noise. I could not break it by the eye into successive discharges, and I am not sure whether it is a perfectly continuous discharge or not.

2903. On diminishing the supply of Electricity this glow could be converted back into brushes. Or on stopping the machine, as the conductor discharged itself by the surface, the glow changed for a moment into brushes before the discharge ceased.

2904. If the hand or a large inducing surface were held opposite the glowing end, the glow could be changed into brushes or the successive discharges. On the other hand, if the brushes were issuing from the Pos. surface or end, opposing a fine point to it could change them into the glow.

2905. These effects very important, for it is evident that the brushes and the glow are two distinct modes of discharge. When the glow changes into brushes, it is because induction more active and so successive charges and discharges occur. When changed into the glow, it seems as if the intermitting action were overruled and a continuous discharge obtained.

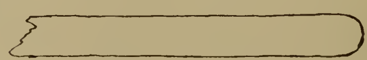
2906. When the glow was changed into brushes by the gradual approach of the hand, it seemed as if the glow gathered itself together on the middle part of the face of the wire, then threw out a point producing the conical foot stalk and this in the gradual progress of the effect produced the ramifications.

2907. With a smaller wire[1] the brushes came on sooner and then the glow, etc. all as before, but with lower intensity of electricity. The size of the brush and the general scale of appearance was smaller.

2908. With a smaller wire[1], still the brushes and then the glow—

[1] The diagrams have been reduced to $\frac{3}{4}$ scale.

* [2901]



but the brushes small, not more than half an inch long and pale and the glow more contracted and like a spark.

2909. With a blunt metallic point could not obtain the brushes, unless the hand or other large inducing surface were used. These brushes were soon changed into the glow and the glow was a mere point or spark more intense in light than the glow on the end of the large wire.

2910. The brush which could be obtained by induction of the hand, etc. on this wire point was small and dull and its sound very feeble, but still it was in nature exactly like the larger and largest brushes and intermitting. It was easily converted back into the glow by an opposed point held in the hand.

2911. The smaller the discharging surface the sooner does the brush pass into the glow or star—induction against large surfaces being equally avoided.

2912. It is necessary in all such experiments to take care that the head, hands, etc. and neighbouring bodies and walls do not interfere in their action. Also that the wires, etc. project out well from the conductor into free space.

2913. With a fine needle point (always positive) I could not distinctly obtain the brush, only the glow or star—but the star bright though small.

2914. The brushes I can always resolve by the eye into intermittances, but the star I cannot. Must however try this by revolving mirror.

2915. Believe that brush and glow approximate and at last on a fine point coalesce, the brush becoming smaller and smaller and recurring more and more frequently until it passes into the continuous state of the star or glow.

2916. Now the brush, whether the coarse one of a large ball or the fine continuous (apparently) one of a rounded wire end, streams to the fingers or a ball held opposite and near to the discharge in a very beautiful manner, bending and shewing well the curvature of the lines of the forces concerned (induction round a corner, etc.); and as the fingers or other things are held nearer and nearer or on this or that side the brush will vary, becoming more restricted or dividing into two streams, or exist in one part as brush and in another part, i.e. against the coarse point, as spark

even, and so that the transition from spark to brush may be made as gradual and regular as can be required: proving that both are essentially the same phenomenon. This also I have often seen to be the case with the long brush or spark from the large machine.

2917. The spark and the brush are the same thing in different degrees of intensity. But the glow or spark [? star] appears to be a distinct effect: though even that and the brush appear to coalesce (   ).

2918. When the needle point was covered with a little tallow, then it could easily be made to give good brushes towards the hand or even into the free air.

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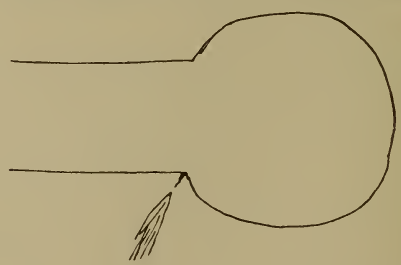
2919. Now had the large cylinder machine put up in the Managers' room so as to be able to obtain Pos. and Neg. Electricity at pleasure.

2920. Repeated the experiments of yesterday at the Positive conductor and obtained the same results.

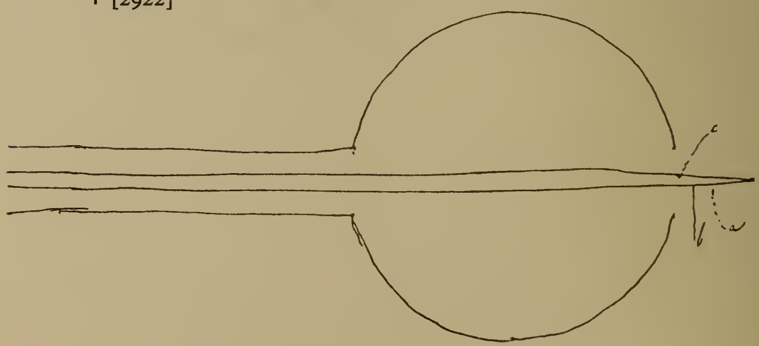
2921*. When a point is under the influence of neighbouring parts of its own conductor, then it assumes the condition of a blunter point. A fine needle point placed in an angle of the prime conductor shot out a beautiful pencil of brushes, with the rushing noise, when the finger was approached to it. But the brush was very much compressed by the influence of the neighbouring parts in a similar electric state. This a beautiful form of induction.

2922†. The point in ball being fixed on the prime conductor shewed this well. When the point projected as in the figure, it gave no brush, only a spark or glow. When retracted as far as *a*,

* [2921]



† [2922]



could by induction of the hand obtain little brushes, easily breaking into the glow. When in as far as *b*, obtained stronger and more noisy brushes, easily separable by the eye. When the point was a little within the curve of the sphere, could obtain excellent brushes 4 or 5 inches long, passing into ramifying sparks and very distinct from each other. Using more electric force, these became either spark entirely—or spark at point and brush at the opposed hand or knob—or at pleasure all brush. Indeed all the varieties of transit from brightest spark to the faintest brush could be obtained at pleasure.

2923. When the point was still further retracted into the globe, the discharges were from the edges of the hole and little or nothing from the point.

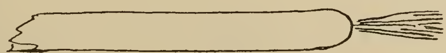
2924. A point held in the hand towards the ball acted by induction as before. When the hand or a metallic ball equal to the hand was held opposite, there was a difference, the ball producing sparks when the hand could produce brushes from the P. point. This is probably partly due to the hairs and irregularities of the hand, and partly to its worse conducting power.

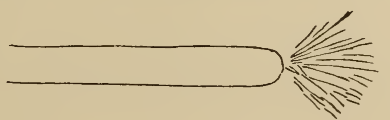
2925*. When a rounded wire end was held opposite to the Pos. ball and point, a brush issued from this negative surface, but it was straight and compressed, it was noisy and rattling in its sound and it could be separated into successive discharges by the eye.

2926. Now proceeded to experiment with terminations rendered NEGATIVE *by communion with the rubber*, not by induction.

2927†. Put the largest wire on to the N. Conductor and worked the machine, the P. Conductor being uninsulated. A brush was formed from the end towards the free air, but it was short and not to be compared with the large ramifying brush when the same wire was Positive ( ). It had a constant noise and was resolvable by the eye into very quickly recurring discharges. In fact for the same termination and the same supply of electricity, the discharges, when the end is N., are probably from 5 to 10 times as numerous as when it is P. Hence a relation to the differences in the size, appearance and sound of the two brushes.

2928. No quickness of working the machine could make this brush pass into the glow, which was observed on the same end

* [2925] 

† [2927] 

when Pos. (   ); it only increased the number of discharges and heightened the pitch of the general sound in consequence.

2929. When the hand was brought near the end of the wire, the tone was raised, the number of elementary discharges being increased.

2930. The brush did not issue indifferently from the general surface at the end, but a bright point or part appeared in one spot and from that the brush broke. The place of this root could be varied by holding the hand on this or that side of the end, the brush always tending towards the hand. The bright point and its brush appear to be the same in relation as the larger bright base and brush of the Positive extremity.

2931. On opposing a point to the Neg. brush, it affected it, diminishing its size and sound, and at a certain nearness these disappeared altogether, leaving the *N. end silent and dark*, but the inducing P. point had the small bright continuous star.

2932. When the fingers were held near this Neg. end brush they did not affect the brush in the beautiful manner before occurring (   ), nor did good brushes go from the fingers.

2933. When a ball was held opposite this N. end brush, no brush formed on it (rendered P. by induction) nor any quiet light; at last when sufficiently near a spark passed.

2934*. When a smaller surface, i.e. the round end of a smaller wire, was brought up towards the N. end and brush, when nearly eight inches off the quiet glow appeared on the surface Pos. by induction, the brush and sound at the N. end combining. As the P. end was approximated, the pitch of the sound of the N. brush rose—at last, continuing the approach, the P. end began to throw of[f] ramifications and distinct brushes and at the same time the N. light contracted and collected together and synchronised with the P. ramifications; and so, at a certain distance, the brushes were formed and apparently determined on the P. surface, but these diminished in brightness in the interval and at or towards the N. surface brightened again, giving there the contracted elongated brush.

2935. A smaller wire end used became luminous with glow at 10 or 12 inches, with no sound, whilst the principal and Neg. surface was sonorous. Being brought nearer, the induced P. surface

* [2934]

continued for some time glowing and quiet, but at last the P. surface gave brush and sound and the brush synchronised with the Neg. brush and both looked alike. With a higher certain work of the machine, the brush and light would disappear at the Neg. Surface and the glow come on at the positive surface, and this state by more electricity would pass into the spark by sudden transition.

2936*. A blunt point was brought opposite the same negative wire end. At a distance, the induced P. point was luminous and silent; the N. surface noisy with a short brush. A little nearer, N. would become silent and cease to be luminous and P. would give small brush resoluble by the eye into independant discharges accompanied by a low sound. By withdrawing the small wire a little or by increasing the quantity of electricity, the P. end or point would return to quite luminous state, and *at the same moment*, N. resume its short brush and sound. The discharges at N. here are very much quicker and oftener than those at P.

2937†. A fine point brought up opposite the Neg. surface of same wire end. The point was always luminous, i.e. within 2 feet or more, and not with brush. As it was brought nearer to the N. surface it affected its luminous brush and sound, and the nearer it was the less sound and light was there at N. end. It does not appear that this large N. surface or end of wire can glow without brush or noise as it can when made Positive.

2938. Now used a smaller Negative surface. The effect was generally as before; thus there was no light without short brush and noise. No increase of electricity could reduce this into a *noiseless glow* as with Positive surface (   ).

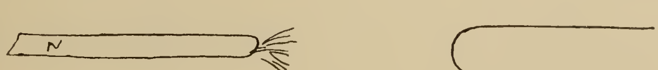
2939‡. A large surface, Positive by induction, was brought up. No glow was produced on it or other change until it was so near that it caused sparks from one to the other: but no brush ever occurred on P. or cessation of the sound and short brush on N. before the spark.

2940§. Brought a blunt point opposite. At a distance there was the silent glow on the induced P. point and noisy brush on N. Nearer, the noise and brush on N. disappeared and a low toned short brush appeared on P. On increasing the electricity the

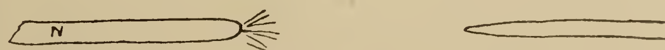
* [2936]

† [2937]

‡ [2939]



§ [2940]



noise and brush recurred at N. and a silent glow on P. Nearer still, the noisy brush at N. and glow at P. increased into brushes which conjoined, and so passing into each other determined their simultaneous and isochronous recurrence.

2941 *. With a fine point opposite the same N. termination, a glow appeared on the former without any brush or noise. When rather nearer, the brush and sound on the end surface was reduced to nothing and no light. Still nearer, a hazy conjoining compressed brush appeared on both.

2942. A fine point rendered Pos. by induction, brought up to N. surface the wire, rendered its brush and noise less even when at great distance. Being brought up closer, it rendered it dark and quiet until quite close, *glowing only itself.*

2943. Now used a coarse N. point or the rubber conductor. There was still a minute sonorous brush as before. The hand or a large surface opposed did not alter it even up to contact, except to produce at very small distance a spark. A fine point opposite to it, being Pos. by induction, was luminous at a considerable distance. When near, it did not destroy the light and sound of N. but only tended to produce a brush from itself. When still nearer a short spark passed.

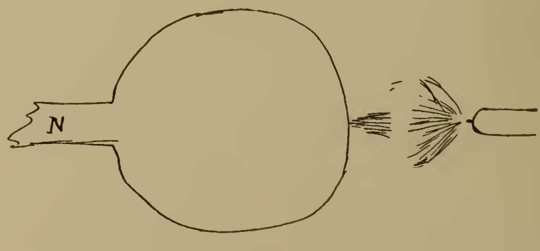
2944. A still finer point used for induction, and being Positive produced the same effects; when very near it produced a spark about $\frac{1}{8}$ of an inch long.

2945. A needle was now made the point of the *negative conductor.* It gave always a spark or star not sensibly sonorous and always luminous. A positive pointed needle opposite to it also had a star almost up to contact and then a minute spark and discharge. The two constant stars were not discernible the one from the other. No sensible brush on either but probably the star might be resolved into minute and rapid brushes on the Neg. surface. As to the Positive surface, the question a little different.

2946. The brush on a surface rendered Positive by induction is not nearly so fine and strong as that on the same surface rendered Positive directly from Prime conductor.

2947 †. When the chief Neg. conductor was a large brass ball and the induced Pos. surface the end of the thick wire or rod, then

* [2941] 

† [2947] 

fine brushes appeared on the induced P. end of wire and N. was either not luminous or with a compressed pencil brush very different from the P. brush in form, etc.

2948. When induced, P. was a fine point; it gave a star at large distances, at certain smaller distances a brush feeble in its nature and easily passing into the discharge and spark. Nothing appeared on the Negative large surface.

2949*. When the large ball was made Positive and the large wire end Neg. by induction, the latter had the short high-toned brush and kept it until within $\frac{1}{2}$ an inch of P. ball, and then sparks occurred. No brush ever appeared on P. and longer sparks than $\frac{1}{2}$ inch could not be obtained.

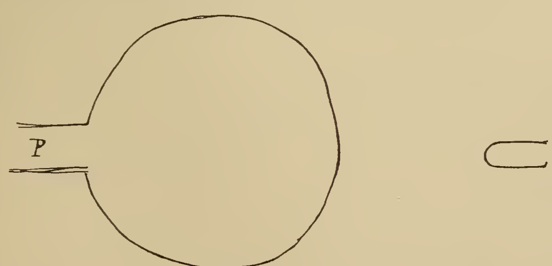
2950. It would seem as if a small N. surface could discharge a large P. surface much better than a small P. surface does a larger N. surface. Electricity appears to break off more readily and freely from N. than from P. surface, but am not sure of this. Is the reverse—see on.

2951. With the *large ball Pos.* and a fine point Neg. by induction, there is always light on the latter (a star) and no brush appears up to the least distance from the *P. ball.*

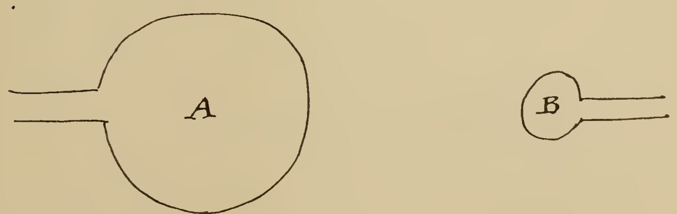
2952. With the large ball Neg. and the same fine point Positive by induction, there was a brush on the point at $1\frac{1}{2}$ inches distance. This brush at P. determined a continuation of it to the N. surface. On bringing up the point, sparks $\frac{1}{8}$ of an inch or more in length could be obtained. This made a striking difference between the two and this is *the essential difference.*

2953†. If two balls be used, one large and the other small, if A the larger be Neg. and B the smaller be positive, then the longest sparks can be obtained; if A be positive and B negative then the shortest sparks are obtained. But I am not yet so sure that the electricity passes more easily in the one case than in the other.

* [2949]



† [2953]



2954. This will probably be the best arrangement in different gases. This also bear upon the investigation of the whole being an inductive effect.

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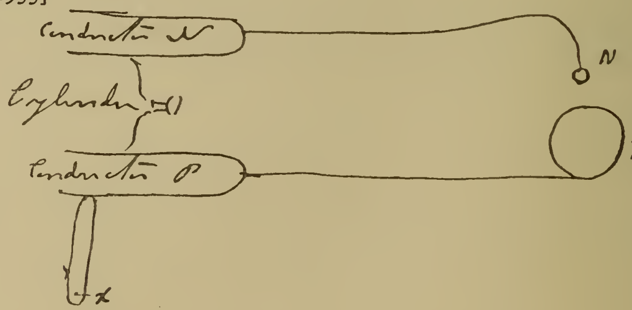
2955*. Now used two brass balls, insulated and connected by wires with the insulated conductors of the machine, so that one could be made positive and the other negative on working the machine. The one ball was $7\frac{3}{8}$ inches in diameter, the other only $\frac{7}{8}$ of an inch in diameter. A smaller insulated conductor was connected with the prime conductor and on it at *x* was fixed a Henly's quadrant electrometer.

2956. In the first experiment the large ball was made Positive and the small ball Neg., and being two inches apart, on working the machine, the electrometer rose to 40°, at which time the brush and sound occurred on the little N. ball and the discharge went on, preserving the electrometer very steadily at 40°. Hence that the intensity required to cause a discharge between these balls, so electrified and at that distance.

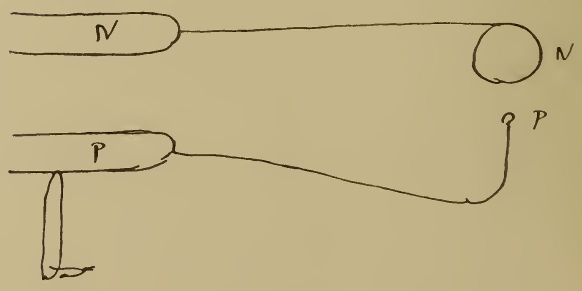
2957†. The balls were now made to change sides but the distance and other circumstances preserved. But though the large ball was now N. and the small one Pos., still on turning the machine the electrometer rose as before to 40°, at which intensity the discharge commenced and there it was retained. On quickening the work of the machine, the P. ball gave first branches and then sparks and thus the electrometer danced up and down much.

2958. Was not quite sure in these two experiments of the results and the discharges.

* [2955]



† [2957]



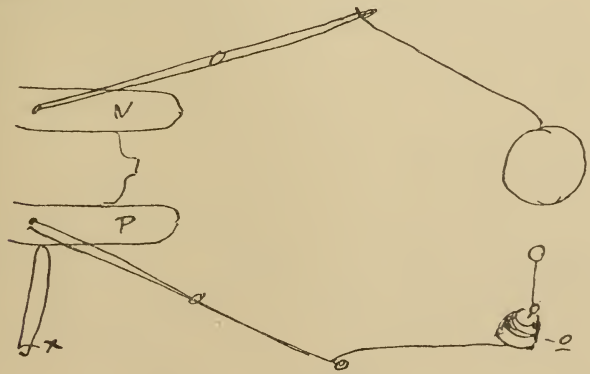
2959. Made another arrangement as to connecting parts thus*, so as to carry the connecting wires more out of the influence of induction. The large ball was $7\frac{3}{8}$ inches, the small ball only half an inch.

2960. A Leyden jar was introduced into this arrangement at *o* for the purpose of testing whether any electricity passed from ball to ball before pencils, brushes, or sparks occurred, for thought it not impossible there might be a passage in some quiet unluminous form. On turning the machine, but not so much as to cause brushes at little P. ball, found that the Leyden jar received no charge, but that immediately brushes were produced or any luminous or sounding phenomena, the *jar did charge*. Hence proof quite delicate enough for these experiments, that there is no sensible previous quiet transmission. This fact good so far.

2961. Removed the jar but retained the large ball Neg. and the small ball Pos. The distance between them was 3·55 inches. At this distance all was silent and no discharge occurred between the balls below or at 40° of Electrometer. At a little quicker working to compensate for the general loss elsewhere, the electrometer rose to about 45°, being rather unsteady there, and brushes occurred on the small P. ball. Working very quickly, so as to have abundance of brushes and so compensate the irregularities of all as well as might be, I still found the average divergence was to about 45° or rather less.

2962. Found that with both conductors insulated, that if whilst the machine was working I touched the Pos. conductor and then took my hand away, the electrometer connected with it remained down for a while, though the sparks or brushes passing between the balls; and that if I touched the Neg. conductor the electrometer rose very much under the same circumstances, and only gradually came to true medium position. This very natural but shews that I must have only one insulated at once.

* [2959]



2963*. The new arrangement is this. The large ball L is permanently fixed and connected with the conductor by a jointed conductor and a wire. It is also connected with the smaller conductor carrying the Henly's electrometer. A wire *w* coming from an excellent metallic discharging train is brought into the room and connected with the small ball, which now becomes electric entirely by induction from the large ball. A branch wire *d* serves to uninsulate entirely the conductor of the rubber. I shall now be able to make the large ball ($7\frac{3}{8}$ inches in diameter) or the small ball (of $\frac{1}{2}$ inch diameter) Positive or negative at pleasure, the other becoming the reverse by induction only.

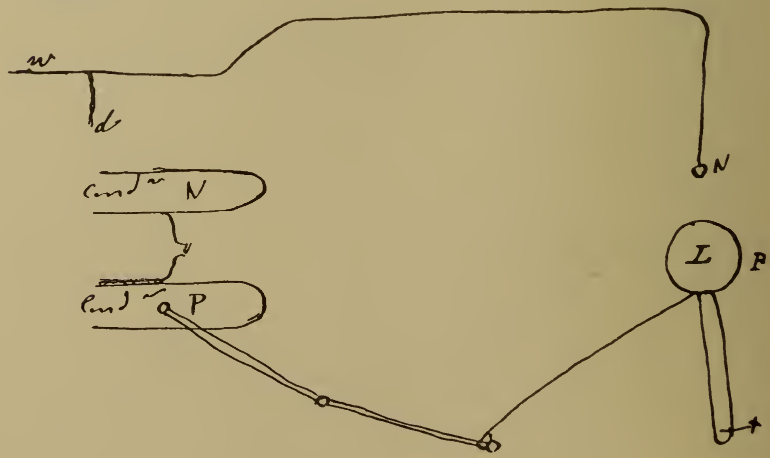
2964. In the state represented, when the balls were separated $3\frac{1}{4}$ inches, Electr. was 80°; it continued there at smaller distn.

at $1\frac{13}{16}$,, ,, 70°
$1\frac{2}{16}$,, ,, 60°
$\frac{10}{16}$,, ,, 50°
$\frac{5}{16}$,, ,, 40° always sparks.

When the distance had diminished to $\frac{8}{16}$, the brush on the N. small ball became chattering in sound and sometimes a spark passed. After each large brush or after a spark the electrometer of course fell. By giving a slow motion to the machine, I could lift the Electrometer to 45° before the chattering brush occurred, and when by quicker motion the brush was continued with a constant sound, still the Electrometer remained a little lower. In fact this ought to be the case, for the height ought to correspond to the medium intensity due to that degree at which it stands the moment before a brush or spark and that to which it falls immediately afterwards.

2965. Could so work the machine as to produce almost at pleasure either a spark or a brush. Now the *intensity* just before the occurrence of either *was the same for both* and this very important.

* [2963]



2966. But there is this difference between a spark and a brush. The former *discharges all the accumulated electricity*, *the latter only a part*. The smaller the elementary brush the less portion does it discharge.

2967. In cases where it is almost spark and brush, there the greatest portion is discharged. But I suspect that, except when it is spark throughout the whole interval, the whole accumulation is not discharged (*for a constant distance*) and that unless the whole is discharged, there is never a spark throughout, the two being constantly associated effects. But must ascertain this point more exactly presently.

2968*. Now altered the arrangement thus: the P. prime conductor was uninsulated. The N. conductor was connected with the large ball which was thus rendered Neg. and the small ball became Pos. by induction.

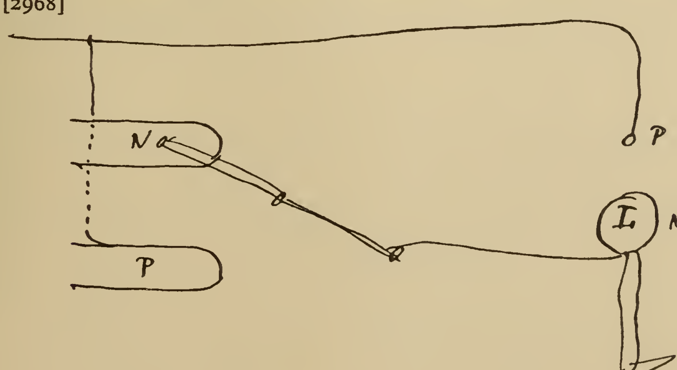
2969. At $3\frac{1}{2}$ inches distance the Electr. was above 80°. There was no ready discharge but on working the machine quickly, sometimes a chattering brush and sometimes a spark discharge was obtained. But as before *both occurred at the same intensity*. The appearance of the chattering brush was something of this kind.

At $3\frac{8}{16}$ inches, 80° or more, chattering brush now and then and also spark.

$2\frac{10}{16}$ . . .	80°
$2\frac{4}{16}$ . . .	80° rarely brush, mostly spark.
2 . . .	78°
$1\frac{8}{16}$ . . .	75° before brush or spark obtained—could get both.
$1\frac{3}{16}$ . . .	70°
$\frac{8}{16}$ . . .	50° always sparks.
$\frac{5}{16}+$ . .	40° do.
$\frac{4\cdot6}{16}$ . . .	38° do.
returned to distance of	
2·0 . . .	80° rather above.

2970. In the higher divergences these numbers come something short, for distance, of those obtained when large ball P. and

* [2968]



small N.; but must have more experiment and learn the necessary precautions before a conclusion drawn; returned things therefore to their former state, i.e. with the *large ball Positive* and the *small one Neg. by induction.*

2971. Now found that when the distance was 2 inches the electrometer was above 80° just before a brush was formed; and that when machine was quickly worked it fell to a little below 78°.

2972. Found here as before that the medium intensity for rapid discharges was always a little lower than that existing just before a distinct discharge (2989).

2973. Thus it appears so far that whether small ball (of $\frac{1}{2}$ inch diameter) be made *Pos. or Neg. by induction*, its power of discharge as judged of by the required intensity is the same.

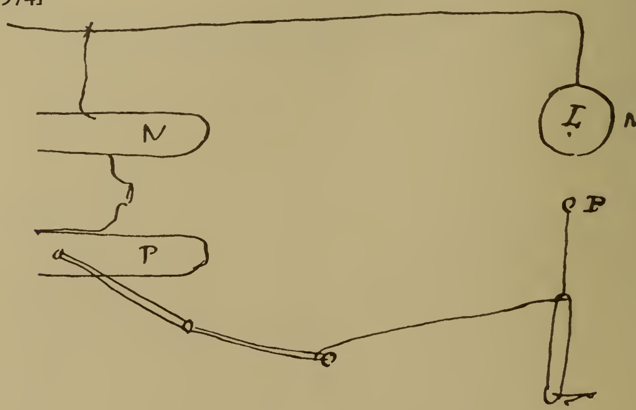
2974*. Now the arrangement was made such that the small $\frac{1}{2}$ inch ball was always *primarily electrified* either Pos. or Neg. and the large ball rendered electric by induction. First the small ball was made *Pos.* and gave strong chattering brush into free air, the Electrometer at 88°:

at 12 inches Electr. 85° about–strong smell of P. Electricity.
$4\frac{2}{16}$ „ „ 80° or above–constant brush.
$2\frac{2}{16}$ „ „ 75° continual burr, passing into chattering brushes.
$1\frac{10}{16}$ „ „ up to 75° before discharge, but dropping suddenly on the occurrence of a brush or spark. If machine worked hard, sparks pass and the Electrometer quite unsteady and bad.

At $1\frac{4}{16}$ inches Electr. 70° before brush or spark–sparks frequent.
$\frac{10}{16}$ „ „ 60° always sparks.
$\frac{6}{16}$ „ „ 50° Do.

2975. Now made the *small ball Neg.* and the large one Pos. by induction–other things remaining unchanged–except that I now

* [2974]



went through the same distances as those just recorded but in the reverse order.

$\frac{6}{16}$ of inch Elect. 58° nearly—certainly higher than when small Pos.

$\frac{10}{16}$ „ „ 65° just before brush—but on brush occurring fell to 51°, the machine being continued at work. This falling effect was constant here, i.e. at this distance.

$1\frac{4}{16}$ „ „ 77° before chattering brush: on keeping a continuous brush was 65°

$1\frac{10}{16}$ „ „ 77° before brush—with constant brush and sound falls to 70°. See 4322.

$2\frac{2}{16}$ „ „ 80° before brush: settling to 72° or 73° with constant brush

$4\frac{2}{16}$ „ „ 80° before discharge—by brush settled to 75° about with continuous discharge.

In air alone the electricity breaks into a brush at about 85° and sinks a little if the brush continued.

2976. I find a point in the hand an excellent thing to bring up electrometer steadily to the discharging intensity. Jerks and irregularities are thus extremely well avoided.

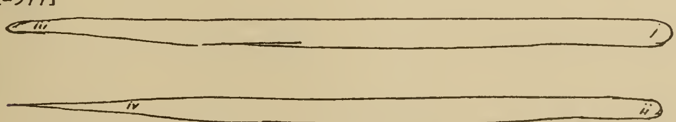
15 FEBY. 1836.

2977*. Have had two long thick wires or rods of brass worked so as to give me four terminations, three hemispheres of different diameters and the fourth a point. The wires are 18 inches long, and the ends numbered i, ii, iii and iv are of the following diameters—

End i	. . .	0·25 of inch
„ ii	. . .	0·20 „
„ iii	. . .	0·175 „
„ iv	. . .	point

2978. To-day made an induction expt. with the end i ( ) and the large brass ball ($7\frac{1}{4}$ inches in diameter) of the nature of the last expt. i.e. the ball and the rounded end were each made Pos. and Neg. in turn and the relative *intensity and distance* observed at different discharging positions.

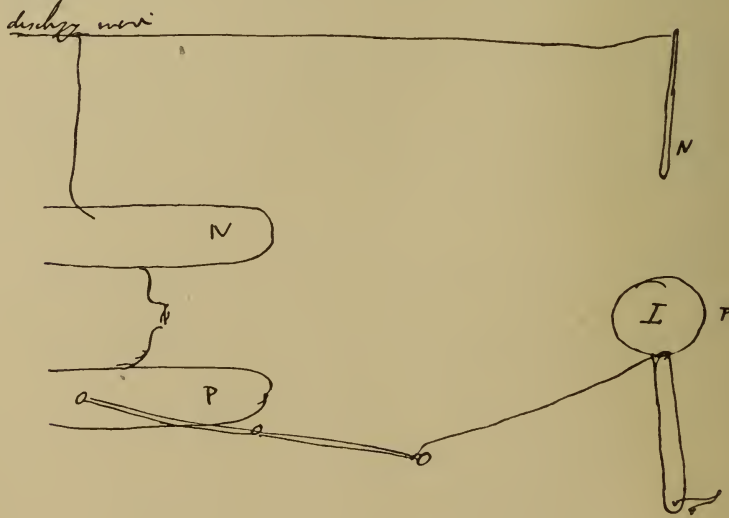
* [2977]



2979*. The large ball was first electrified positively, being arranged as in the experiments of 11th Feby. ( ). The approached rod was of course connected with the good discharging wire and the rubber N. Conducter also uninsulated. In the first instance, the rod N was not present but the elevation of the Electrometer was noticed, the large ball L being alone and the wires connecting with it being of course the dischargers into the air. The Electrometer then stood at 90°. Then the distances were made:

		90°	
11	inches	88°	the buzz of discharge sound only heard when machine quickish.
10	,,	87°	had more continual discharge sound.
8	,,	85°	steady at 85° with sound.
7	,,	83°	Do.
6	,,	82°	Do.
5	,,	80°	There seems a little sinking of Electr. as discharge continued. See 4322, etc.
4	,,	78°	Do.
3	,,	76°	Do.
2	,,	72°	Do.
1·5	,,	65° +	
1·0	,,	58°	
0·75	,,	52°	
0·50	,,	45°	Down to this point only brush, but sound gradually louder.
0·25	,,	34°	Could obtain by slow motion *brushes* or by quicker sparks, but both came on at the same intensity of 34°.
0·125	,,	17° or 18°	and always spark.

* [2979]



Observed in this experiment the quick soiling of the surface of the ball and also end by the attraction and adhesion of dust to their electrified surfaces.

2980*. Now shifted the flexible conductor so as to attach it to the N. Prime conductor and uninsulated the P. Prime conductor. The large ball was therefore Primarily electrified *Negative* and the same end as that just used electrified *Positive* by *induction*. Then exactly the same intervals as to distance were proceeded through as before, but in the inverse direction, and thus the two tables are easily comparable.

0·125 of inch 17° or 18° sparks–as nearly as possible the same as before.

0·25 „ „ 34° sparks.

0·50 „ „ 45° sparks–no brush obtainable.

0·75 „ „ 52° brush if very slow–the *glow* if quick, and then steady at 52°. No spark any way.

1·00 „ „ 58° with *glow* and electr. steady–but if machine worked slowly so as to give brushes, which were easily obtained, then much dancing of electr. as if sparks were passed, but not to the same degree. The steadiness of *glow discharge* very beautiful.

1·5 „ „ 64° with slow motion of machine brush now and then, but not so chattering as at 58°. If machine a little quickened, the glow soon came on and a steady discharge at 64°.

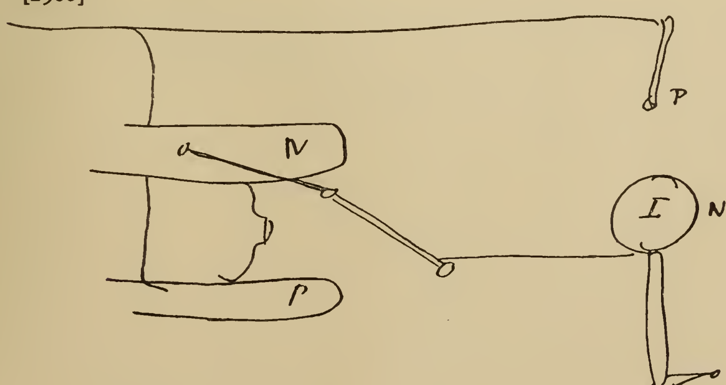
2 „ „ 70° the same either for the *brush* or *glow*–glow most facil.

3 „ „ 76° with brush and slow motion } could keep either
79° with glow and quick motion } constant.

4 „ „ 82° with brush and moderate motion.
85° with glow and hard work.

Here preserved the distance unaltered but made large ball L Positive and end of rod N. by induction–the electrometer now

* [2980]



rose to 84°. Hence intensity of discharge the same whether the ball is made primarily *Positive or Negative*.

Did the same thing occasionally in the future results

L pos. 5 inches 82° constant burr of N. brush from end N.
L neg. 5 „ 90° with brush—then fell a little—being worked hard, rose up to 90° with glow—relaxed working and then Electr. fell to 84°.
L neg. 6 „ 90° either brush (or with very hard work) glow.
L pos. 6 „ 90° neg. brush and sound—by continuance fell to 88°.
L neg. 7 „ 90° pos. brush generally—could just obtain *glow*.
. . . 8 „ 90° Do. . . . but still could just get glow.
10 „ brush allways with this machine quantity.

2981. I now changed the places of the rod and the ball, i.e. the same rod end i was used but was primarily electrified and the large ball assumed the opposite state by induction. Now also after the effect at a certain distance had been noticed, the electric condition of the rod end was changed by communication with the other conductor and an observation again made. Hence the effects when the end was P. and N. were both observed before the distance was altered or the condition of the machine changed. This I will express by putting E. pos. or E. neg. before the observation.

E. neg. into air 77° began to brush—rose to 92° with hard working.
E. pos. „ „ 77° brush began, continued till near 90°, then glow came on.
pos. 12 inches 74° Do. . . . . 85° Do. once glow occurs no higher rise.
neg. 12 „ 74° brush began, could raise Elect. to 90° pitch of sound rose, *no glow*.
neg. 10 „ 72° Do. . . . . 86° Do.
pos. 10 „ 74° . . . . . continued till near 85°, then glow came on.
pos. 8 „ 71° . . . . 83° then glow, dancing Eltr. with brush.
neg. 8 „ 72° . . . . could raise Eltr. to 87°, pitch of sound rose. No glow.
neg. 6 „ 68° . . . . 80° . . .

pos. 6 inches	69° brush began, could raise Eltr. to 80° then glow dancing with brush.
pos. 4 ,,	66° . . . . 70° and then glow, which continued until Electr. near 80°. When glow comes on, there is always a little sudden elevation of the Electrometer.
neg. 4 ,,	66° brush began and continued to 77° pitch of sound rose—no glow.
neg. 2 ,,	60° . . . . 64° the rise less at short distance.
pos. 2 ,,	60° steady at 60° with brush; with little more work became glow, jumping up to 63° and rising by higher work to 65°.
pos. 1 ,,	49° with brush—glow came at 51° and electr. continued there.
neg. 1 ,,	47° pretty steady, could raise Electr. to 48°.
neg. 0·75 ,,	45° steady at 45° with quick or slower work.
pos. 0·75 ,,	45° brush soon passes into glow, but still steady—it was always below 46°.
pos. 0·5 ,,	37° *glow* or *spark* but *no brush*—could rise continuing the glow up to 40° and sink again with it to 37°. Could obtain either sparks or glow at pleasure, but that which was tended to continue. Thus if a spark occurred then a fit of sparks ensued, or if the glow was on a fit of glowing continued.
neg. 0·5 ,,	38°—brush—no sparks or glow—if machine worked harder, the pitch of brush sound rose, indicating more rapid occurrence of discharge, but the electr. did not rise higher.
neg. 0·25 ,,	25° always sparks—by increasing the distance a little I could obtain either sparks or brush at pleasure.
pos. 0·25 ,,	25° always sparks—by increasing the distance a little I could get the glow but not the *brush*. If the glow were on and the machine worked hard, I could diminish the distance, preserving *the glow* until the distance was only 0·25—when *beginning* at 0·25 I could only procure sparks.

2982. With respect to the main point of whether the intensity is the same for the same small discharging surface whether it be P. or N., it appears *that it is*, or that the irregularities are due to some other cause.

2983. Tried to obtain a glow upon a large surface rendered Positive by a Neg. point very near to it, but obtained nothing of the kind. In fact it could not be, for a Neg. point will take down glow and cause its disappearance on the end i just experimented with.

2984. Made the end i positive and then by working the machine well and approaching the hand obtained the glow or the brush on it at pleasure. I found the glow gave a powerful direct wind, but that the brush was much less windy. There was a very strong current of air set off by the glowing wire end. Hence not likely that a ball would glow, or at least only on one side.

22 FEBY. 1836.

2985. Made similar experiments to those just described using the large ball ($7\frac{1}{4}$ inches in diameter) and the brass point iv. First of all the ball was primarily electrified P. and N. and the point electrified N. or P. by induction. The connection of the ball was every second observation changed from N. to Pos. or P. to N., so as to allow the effect to be observed both ways at the same distance.

	Ball P. Electr.			Ball N.
In air ...	84°	Star visible on point in dark.	in air ...	83° star
13 ...	80°	Star visible on point.	13 ...	79°
11 ...	77°		11 ...	70°
9 ...	75°		9 ...	67°
7 ...	67°	Electr. very unsteady.	7 ...	60°
5 ...	50°	Electr. continued unsteady.	5 ...	50°
4 ...	35°		4 ...	35°
3 ...	20°		3 ...	25°
2 ...	12°		2 ...	15°
1 ...	5°		1 ...	2°

2986. Great irregularities exist here—there were also great un-

steadinesses of the Electrometer with same distance and state. Working machine slow or quick would make 20° difference. Working it quickly caused great vibration because electrical cylinder not true on its axis. Again the P. and N. prime conductor not equally well insulated and charged. But the general result is, I think, that whether the point is P. or N. by induction, the power of discharge through air is the same and the required intensity therefore the same.

2987. Now made the Point primarily P. and N.

Point P.	Point N.
	in air ... 60°
in air ... 75° sank to 70°	in air ... 65° sank to 60°
16 ... 65°	16 ... 55°
12 ... 50°?	12 ... 50°
8 ... 45°	8 ... 30°
6 ... 30°	6 ... 25°
4 ... 15°	4 ... 10°
2 ... 7°	2 ... 5°

2988. Think that much of the difference between the two series generally depends as before upon difference of the excited condition of the P. and N. prime conductors. Any effect of that kind comes serious in here where there is a point always throwing off into the air and keeping the electricity down. As the working the machine more or less quickly makes a serious difference in the intensity on the conductor, it with many other facts shews that there is a resistance (considerable) to the discharge, but it shews also that time is an element, for it can compensate for that resistance and suffice for the discharge, even at much *lower intensities*.

2989. When the point electrified in the air, there is this peculiar and striking effect. Suppose it *positive*. The intensity will rise to a certain height, as 75°; but continuing to work, it will fall to 70° or 65°, as if point discharged more freely. Then make the point *Neg.*: the intensity will rise to 65°, but continuing to work will fall to 60° or 55°. Again make it Pos.: it will rise to 75° but gradually fall again and so on continually—as if, by continuing the discharge from a point, it became more able to effect that discharge—acquired an aptness for the purpose, but on the

contrary became more inapt for the discharge of the opposite electricity.

2990. I thought I saw something of this kind also when using the rounded ends of the wires (2972).

29TH FEBY. 1836.

2991. Examined the light of Positive and Negative terminations by Wheatstone's revolving mirror.

2992. Ends i, ii and iii ( ) rendered Positive. Whilst they gave the brush, attended by sound, the light was resoluble by the revolving mirror into distinct discharges; but not more readily, I think, than when the eye alone was rapidly moved across them.

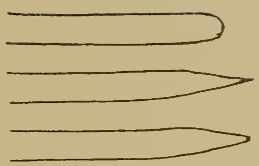
2993. When the glow was on these points, I could not resolve the light either by the eye or the mirror. In the mirror the circle of light formed was exceedingly faint.

2994. Point or end iv Pos. gave its small star of light always and this I could not resolve.

2995. I do not think that under the different forms of stars and brushes and glows from these different ends, the light is the same for the same discharged quantity of electricity. The point gives decidedly less than the other ends, and I think the glow less than the brushes.

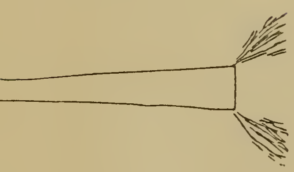
2996. When the ends i, ii and iii ( ) were rendered Negative, the brushes produced, though short, were always sonorous and resoluble; the glow could not be obtained.

2997. When end iv or the point was negative, I could not separate the star into distinct discharges. The light was so little that it was not visible in the revolving mirror—too much diluted to be sensible.



2998. *Wood.* Ends of wood were used. When these were positive, could obtain either brush or glow. The brush was resoluble, the glow not.

2999. When the wood ends were negative, the discharge was always as a brush, shortest and highest in sound with the finest end. The mirror resolved them much about as the moving eye did.



3000. *Paper.* Ends of card projecting beyond the brass conductor about two inches. The angles of the square end and the end of the

pointed piece gave beautiful brushes when the charge was positive, especially if a large inducing surface opposite.

3001. When the paper or card was Negative, it gave a luminous edge extending a little way from the points or angles—it was rather difficult to obtain the negative brush; it was rather the elongated star, i.e. star extending along the edge of the card. Yet small brush could be obtained by management. If the end of the card were touched with the finger, then strong brushes covered the card, extending from finger to conductor; but these brushes originated rather at the finger rendered pos. by induction, the paper being then nearly an insulator.

3002. *Charcoal point* obtuse. Positive, a brush as usual at pos., sonorous—resoluble into successive discharges by the mirror and the eye, and I could not break it into the glow by this machine.

3003. Negative. A small singing brush as usual at Neg. surface, resoluble by the eye and mirror.

3004*. *Nitre*—a crystal fixed by tin foil on to the end of a long wire —about half an inch of crystal projecting rendered *Positive*; it conducts so badly that though there was a little brush from the end, the principal brushes were from edge of foil against the crystal.

3005. When the Nitre was rendered Negative, there was a small star at its extremity, but plenty of starry points of light round the edge of the foil against the nitre. The P. and N. differences of light just as in former cases.

3006. *Citric acid* arranged in the same way—conducts better than nitre. When *Positive*, brush—when *Neg.*, small brush or star.

3007. *Oxalic acid* crystals in same way—conducts badly, Pos. brush from foil round it—Neg. star on foil round it.

3008. *Oxide of Lead*—a piece of fuzed prot oxide in same manner. Not conduct to produce light. Brush and star at edge of foil as before.

3009. *Chloride of lead.* Conducts fairly—all the brush or star at the end of it. When *Pos.*, short brush, sonorous and resoluble. When *Neg.*, star of light scarcely resoluble, but feebly sonorous.

3010. *Carb. Potassa*—fuzed piece—*Positive*, a long linear brush, singing sound, but hardly resoluble to eye or this mirror. When *Neg.*, star of light not resoluble, no audible sound.

* [3004]

3011. *Potassa fusa*—good conduction—Positive, a good brush. Neg., a small brush as with metal—both sonorous and both resoluble.

3012. *Strong solution of Potassa*, i.e. made end of Potassa fusa damp—results as before.

3013. *Strong Oil Vitriol* on end of wood. When Positive, obtain good brushes, but soon passes into glow. When Neg., small brush, star.

3014. *Sulphur*. Not conduct—the edge of the foil discharges.

3015. *Sulphuret of Antimony*—conducts well. *Positive*, it gives good brushes. Neg., small brush or star as with metal, etc.

3016. *Hæmatite*—fibre of—just as metal when rendered Pos. and Negative.

3017. It seems as if variation of the chemical nature of the Electrode made no difference as to the character of the P. and N. discharge into *Air*.

3018. Now experimented with a few edges to observe where the discharge occurs.

3019. *Knife edge*, a case knife, *Positive*. The brushes into the air were partial, i.e. existed here and there on the edge and not generally. They could be determined, in this or that place on the edge, by approaching the finger or other conductor. But it is clear that when one point of an edge is discharging the neighbouring parts are not discharging; that point overcomes the vicinity and determines something like an opposed state there, probably by the condition into which it brings the air by its discharge.

3020. Could easily obtain the glow on the extreme end of the knife and this glow extended a little way round the end, and was *not at a point*.

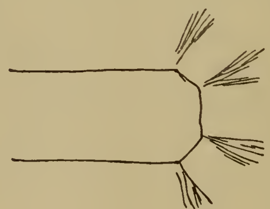
3021. When the knife *Neg*., then starry points, but only here and there—no brushes.

3022*. *Tin foil* edge—Positive. Brushes from corner angles. But if finger brought opposite one, the others disappear more or less.

3023. Rendered *Neg*.—stars at angles. If finger opposite one the others weakened and even reduced to nothing.

3024. *Gold leaf*—became very tense and stretched when electrified

* [3022]



but still the star or glow very limited and only in few places. There only a discharge.

3025. But brushes obtained if a spark determined elsewhere in the course of the electricity—or probably if a piece of badly conducting matter intervene. *Good.*

Date Due
